THE EFFECTS OF PROPAGANDA ON RECALL, RECOGNITION AND BEHAVIOUR

I. THE RELATIONSHIP BETWEEN THE DIFFERENT MEASURES OF PROPAGANDA EFFECTIVENESS

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Two experiments are described in which the effects of propaganda were assessed by the standard forms of recall and recognition tests as well as by any change in overt behaviour following the propaganda. The first, a field experiment, using road-safety propaganda presented in a Children's Cinema Club, showed that there was no positive relationship between the effects of the propaganda as shown by the tests of recall or recognition on the one hand and behaviour on the roads on the other. A second experiment was conducted in the laboratory where an attempt was made to influence a subject's choice of fast or slow speed on a mechanical task by means of incidental propaganda. The 'effect' of the propaganda varied according to whether its influence was assessed by its attention value, its intelligibility, by how much could be described or recognized, or by its capacity to influence behaviour. Some subjects had only attended to individual items of the display and, although they were able to recall or recognize these, they were not able to understand the implication of the propaganda and were not influenced by it. Others understood and recalled the propaganda but were not influenced by it. Finally, there were those subjects who understood the propaganda and were influenced by it but who could not remember it. Thus it seems that the information which has been learned from propaganda can become apparent in two distinct forms: (*a*) in recall or recognition, and (*b*) in subsequent behaviour. The reasons why these different effects were more marked in some subjects than in others will be examined in the second part of this paper.

I. INTRODUCTION

It is the aim of most modern propaganda to influence the action of individuals or groups in some specific way. Experimental studies which have been concerned with assessing its effects have, however, concentrated not on measuring resultant action, but on estimating its influence on some intermediary response such as measuring how well the propaganda can be recalled or recognized or whether it has effected a so-called 'attitude' change as measured by responses given on a questionnaire. It has usually been assumed that propaganda—or in fact many forms of learning—will affect the action of the individual in proportion to the extent to which its content can be recalled or recognized and that a statement made on an attitude questionnaire will predict an individual's behaviour. The present study is concerned with assessing to what extent, and how, the various measures of so-called effectiveness are related—or whether, in fact, they are necessarily related at all.

Experiments on the effect of propaganda have been conducted mainly in two fields of study: first, in relation to Commercial Advertising and Market Research, and, secondly, in the study of attitude change. In the former we find that the effect of an advertisement on buying behaviour has been assessed by anything from the observation of eye movements, tests of recall and recognition, and paired associate tests (Link, 1934; Poffenberger, 1932) to a more recent technique based on the psychogalvanic reflex (Eckstrand & Gilliland, 1948). In 1932, courses were being organized, as a result of these tests, on the psychology of advertising. The methods which were developed then have varied little over

the subsequent years (Gallup, 1950). Attention value is considered to be of overriding importance. Unlike much other experimental work, advertising research has been strictly utilitarian and a systematic study of the theory and methodology has lagged behind. Attempts to validate the tests by relating the results to actual behaviour* are desultory and not encouraging (Jenkins, 1938; Poffenberger, 1932; Welch, 1941). For example, an attempt to assess the extent to which the results of orthodox readership surveys were dependable showed that only 47 % of the subjects actually mentioned a periodical which they usually read (Longstaff & Laybourn, 1949).

The measurement of people's beliefs and attitudes is also assumed to be an adequate prediction of social behaviour. It is in this field of attitude research that the vast majority of experiments on the effects of propaganda have been conducted. That 'attitude' changes (as measured by attitude scales and questionnaires) can be brought about by various means has been amply demonstrated (Chen, 1933; Gardiner, 1935; McCreary, 1953; Smith, 1943). But basic to the whole problem is the extent to which these data are capable of predicting with any certainty the course of action that will follow the establishment of any given attitude. Verbalizations elicited in one social context do not always enable us to predict with accuracy non-verbal behaviour in another. Attitude tests are, in fact, eliciting verbal responses in relation to mental pictures of a situation. The general assumption has been made, however, that these tests are true indicators of both verbal and non-verbal behaviour of people towards actual objects and situations rather than being intellectual or socially conditional beliefs or opinions. Such attempts as have been made to validate attitude tests in the light of subsequent behaviour have shown their prediction to be poor. For example, a questionnaire was sent to hotels and restaurants in the United States by La Piere who had previously visited them in company with two Chinese students. The questionnaire asked 'Would you be willing to accept members of the Chinese race as guests in your establishment?' All but one of the replies was negative. As a matter of behavioural fact, he and his friends had been rejected six months before by only one proprietor (La Piere, 1934). A further difficulty in assessing the relationship between attitudinal change and behavioural change is that, in the traditional pre-test post-test procedure, no account is taken of the effect which the pre-test may have on raising the subject's susceptibility to propaganda. A pre-tested audience is no longer typical of audiences in general. Jarrett & Sherriffs (1953) claim novelty value for their experiment in so far as the traditional pre-test design is rejected in favour of matched groups with a post-test only.

It would seem clear from the foregoing that tests of recall and recognition and measurements of 'attitude' do not lend confidence to the belief that they are capable of accurately predicting behavioural response. Conversely, it has been apparent in fields unrelated to our present study that a behavioural response does not always imply that an equivalent response can be made verbally. For example, the process of inductive concept formation is often accompanied by an inability to give the concept a verbal formulation (Hanfmann, 1941). Similarly, it is a commonplace that, in skilled activity, we cannot recall the manner in which our skill is exercised. In the field of abnormal psychology there is a good deal of evidence that, after simple instructions, correct responses are possible without conscious memory of the origin of the association. The hospitalized Korsakov patient will admit no knowledge of his surroundings although he nevertheless behaves as if he were in hospital

* Throughout this study the term behaviour is used to denote non-verbal action.

(MacCurdy, 1928). Stern reports how a patient suffering from amnesia '...could write shorthand, which he had learnt during the forgotten period, with undiminished skill' (Stern, 1938). There is evidence, too, from the studies of material presented below threshold, that subjects are able to respond to stimuli which they cannot report verbally (Lazarus & McCleary, 1951; Miller, 1939). In none of the experimental studies, however, is there evidence to show the degree to which, or how, these two forms of response are related.

Previous experiments on the effects of verbal and pictorial propaganda have usually been designed in the form

Display of propaganda A.
Recall or recognition of propaganda A.
Display of propaganda B.
Recall or recognition of propaganda B.

The relative effectiveness of the propaganda A and B have then been compared, but the effects of each on action have not been considered. Such experiments follow the tradition of the classical investigations of the learning of verbal material which have been conducted exclusively in the field of memory. What and how a subject learns verbally has been examined by what he remembers. Theories of learning have been built up round the way in which individuals respond to and later report either meaningful or nonsense verbal material. The only studies of experimental psychology which seem to be concerned with the effect of symbols on action are to be found either in the fields of animal and infant psychology (e.g. discrimination experiments) or in a few experiments related to transfer of training in which verbal training is said to assist the learning of a skill (e.g. the effect of mental rehearsal on piano playing investigated by Rubin-Rabson in 1941). Yet whether it is in the social contact of groups or in the use of mass media, the ultimate importance of verbal messages in real life situations is their influence on action. The main attention has been on making learning situations quantifiable. Their application to real life problems has received secondary consideration. It is in an attempt to gain a better understanding of the relationship between the standard criteria of effectiveness and the effect as measured by social behaviour that the present study has been conducted, where experiments have been designed to measure the effectiveness of propaganda on recall, recognition and behaviour.

II. EXPERIMENT 1

(i) *Experimental procedure*

A field experiment was designed to measure the effect of propaganda on children's behaviour on road crossings. This experiment is reported more fully elsewhere, i.e. Belbin (1950*a*, 1953). It was conducted on members of Children's Saturday-morning Cinema Clubs, i.e. on a 'closed community'. Propaganda in relation to the use of pedestrian crossings was presented within one club on three occasions during a year as follows: propaganda A: four film slides; propaganda B: talk by a police sergeant; and propaganda C: a cartoon slide. In another district, an eight-minute children's coloured film was shown and was followed by a slide specific to the local traffic situation (propaganda D).

The following measures of effectiveness were recorded:

(*a*) *Observations of the children's behaviour* on the crossings as they left the cinemas were made throughout the year, i.e. both before and after the propaganda. Observers were posted at each of the crossings and behaviour was recorded as follows:

	Correct use	*Incorrect use*
Pedestrian crossings (3 observers on each)	Those who used 50% or more of the crossing	Those who crossed without using the crossing or who used less than 50% of it
Police-controlled crossings (3 observers on each)	Those who approached the police to be escorted across	Those who went across at uncontrolled points
Traffic-light crossings (6 observers on each)	Crossing when the lights were green	Crossing when the lights were red

The observers were not informed as to when the propaganda would be presented.

Individual behaviour, as well as total numbers, was recorded after propaganda A and B in order to compare it with the amount of propaganda which was remembered or recognized. For this purpose children were handed cards, one colour to those who had crossed correctly and another colour to those who had crossed incorrectly. A slide had been shown, during the club performance, telling the children that these cards would be given to them outside the cinema for a lucky number competition. They were asked to fill in their name, age and school. (Free seats were given to a number of the card holders on the following week.) It was thus possible for the experimenter to follow up individual children to their schools to give them tests of recall and recognition while, at the same time, knowing, by the colour of their card, whether each child had used the crossing correctly or not.

(*b*) *Recall of the propaganda.* Those children between the ages of 7 and 14 years who had received cards at the crossings were visited at their schools on the Mondays following propaganda A and B by teams of research workers and students. They were asked to say what they remembered of various items in the film programme, one of the items being the road safety propaganda.

(*c*) *Recognition of the propaganda.* After propaganda A a recognition test was given to each child who had received a card. After propaganda C, all the children who had visited the Cinema Club on the previous Saturday were given a recognition test on the Monday morning by members of the staff in their schools.

(*d*) *Attention to propaganda.* Photographs of the children were taken during the presentation of propaganda and during the film programmes and were synchronized with sound recordings of the amount of noise made by the children.

The experimental design may thus be represented as follows:

Pre-test (observation of behaviour).

Propaganda.

Post-test (observation of behaviour, measurement of attention, recall and recognition).

(ii) *Results*

(*a*) *Behaviour on crossings.* On the whole, no interesting changes were observed as a result of the propaganda. There was no improvement at all in percentage correct behaviour during the 3 weeks after propaganda A and only on one police-controlled crossing was there any measurable improvement after propaganda B. This latter appeared reasonable, since this particular propaganda was a talk directed towards the use of police-controlled

crossings. However, an even greater improvement 3 weeks after the propaganda suggested that the cause for improvement lay in the identity of the police who were on duty and not in the propaganda. Those who were well-known to the children were easily able to induce satisfactory behaviour on the roads. After propaganda C there was a slight decrease in the percentage incorrect behaviour of the girls but there was no similar decrease for boys. When the percentage correct use of crossings before and after propaganda D is ranked for both boys and girls (Table 1) it can be seen that there was no apparent improvement in behaviour as a result of propaganda. Week 5 does in fact show an expected average value for both boys and girls.

Table 1. *Rank order of percentage correct use of crossings before and after propaganda D*

	Crossing 1		Crossing 2		Crossing 3	
Week	Boys	Girls	Boys	Girls	Boys	Girls
1	4	2	2	1	2	2
2	3	1	4	2	1	5
3	2	5	1	3	4	4
4	5	4	3	4	5	3
			Propaganda			
5	1	3	5	5	3	1

(*b*) *Attention to the propaganda.* Photographs taken of the children during the performance and during the presentation of propaganda offered no criterion of attention. From these photographs there seemed to be less concentrated attention on the propaganda than on the films, especially during the talk of propaganda B. Synchronization of sound recordings with the photography, however, revealed that the apparent attention to the films as judged by the photographs was misleading, since the children were making far too much noise for any of them to have heard what was being said. The children appeared to follow a story visually but were highly selective in attending to the sound track. It is of interest to note that a few children who were identified in the photographs as looking directly at the propaganda slide could neither recall the content of the slide nor recognize it when shown a copy.

(*c*) *Recall and recognition of the propaganda.* 80% of the children who were interviewed were able to describe at least one of the slides of propaganda A. 48% of them described two or more slides. 90% correctly identified one or more of the slides in the recognition test.

After propaganda B, the children were asked to say anything they could about the talk given at the club. Seventy-four of the ninety-six children who were questioned could recall at least one point in the talk which was related to crossing the road at the appropriate place.

After propaganda C (a cartoon slide of a chicken crossing the road by way of a pedestrian crossing with the inscription 'Use the proper Crossing Boys'), a copy of the slide with the crossing and wording removed was sent to seventeen schools. The children who had been to the club were asked if they remembered having seen this picture and if so why it was shown to them and what was written on it. Of the 536 children who had attended the cinema, 160 did not recognize having seen the slide. Of the 376 who said they did recognize it, 153 knew that it had something to do with pedestrian crossings. Only one

child knew the exact five words on the slide. Thus, if a standard form of recognition test had been used as a criterion of effectiveness of the propaganda, it would have been 'effective' to 376 children. When a test of recall was used the propaganda was 'effective' to 153 children.

(*d*) *Individual behaviour in relation to recognition or recall.* An attempt was made to discover whether those who best recognized or recalled the propaganda were, in fact, those who had used the crossings correctly. The high standard of recognition of the slides in propaganda A made it difficult to relate any particular level of recognition to any particular group. Table 2, however, which shows the number of children who expressed no doubts in their identifications, is of some interest. It will be seen that those who crossed correctly and those who crossed incorrectly made about the same number of errors in the recognition test.

Table 2. *The relationship between individual behaviour and recognition after propaganda A*

(Children who expressed no doubts in their identifications.)

	Boys		Girls	
	No. of subjects	Identification errors in recognition test of 9 pictures	No. of subjects	Identification errors in recognition test of 9 pictures
Used the crossings correctly	21	32	17	18
Used the crossings incorrectly	24	37	7	9

From Table 3 it can also be seen that there was no uniform relationship between individual behaviour and recall after propaganda A.

Table 3. *The relationship between individual behaviour and recall after propaganda A*

(Number of children who could recall the slides.)

	Slide 1	Slide 2	Slide 3	Slide 4
Used the crossings correctly:				
Boys (26)	2	15	13	6
Girls (23)	2	9	9	9
Used the crossings incorrectly:				
Boys (32)	4	10	18	6
Girls (11)	3	4	7	7

Similarly, there was no uniform relationship between recall of the talk of propaganda B and behaviour on the crossings after it. Every child who had used the crossing incorrectly was able to recall at least one of the items relating to use of crossings in the talk.

(*e*) *The effects of age or intelligence.* An attempt was made to investigate the influence of two other variables—age and intelligence—on the results so far obtained. First, the question arose as to whether recall, recognition or behaviour varied within the different age or intelligence groups. Secondly, since there seemed to be no change in behaviour large enough to be measurable in the conditions under examination (in spite of the high recognition scores), it was of interest to note whether the children who attended the club formed a selected group, i.e. whether they had particularly low or high I.Q.'s in relation to the rest of the population.

An analysis of the results, in relation to the ages of the children concerned, suggested that, while behaviour on the crossings varied very slightly with age (older children and the 9-year-olds being the worst offenders), there was no general continuous change with age in the recognition or recall scores.

Intelligence quotients were not available from the majority of schools in the area. I.Q.'s (Spearman Group Test) from all members over the age of 11 were obtained from one school, however, and were compared with individual success in the tests after propaganda C. There was no interesting relationship between I.Q. and ability to recall the wording or the purpose of the propaganda.

The I.Q.'s of all the members in the school were analysed in order to investigate whether the Cinema Club members were less intelligent than non-members. In fact, the members and ex-members were found to be slightly more intelligent than non-members (median for members and ex-members = 94·5 and for non-members = 90). Those who gave up membership of the club before reaching the age limit seemed to be the least intelligent of the groups.

(iii) *Discussion*

It was clear from this experiment that there was no uniform positive relationship between recall or recognition of the propaganda on the one hand and behaviour on the other. Individual success in the tests was no criterion of behaviour on the roads. These findings are in conflict with the usually accepted belief that propaganda will affect behaviour in proportion to the extent to which it can be remembered or recognized. It would seem that the standard methods of measuring 'effectiveness' present, on the whole, a vastly exaggerated picture of the powers of road safety propaganda. Some reservation is necessary, however, before arriving at any definite conclusions. Certain variables in the field situation may have masked the ultimate effectiveness of the propaganda. For example, it may have made its mark on the children by heightening their consciousness to potential accident situations without changing their behaviour. Secondly, it has not yet been possible to indicate the underlying causes of the apparent lack of relationship between the influence of propaganda on behaviour on the one hand and the extent to which it can be remembered or recognized on the other. It was thought that the nature and extent of such a relationship could best be studied in the more controlled conditions which are possible in laboratory situations. Further experiments were designed accordingly. The first of these is described below.

III. EXPERIMENT 2

(i) *Introduction*

The previous experiment demonstrated that the ultimate success of propaganda cannot be predicted from tests of recall and recognition. If we are to examine more closely the degree to which and under what conditions a relationship exists between the different forms of response, it will be necessary to study the effects, in greater detail, on individuals rather than on groups.

One of the difficulties of studying the effects of propaganda is to find a suitable task to which propaganda can be applied without imposing conditions of artificiality. Indeed, the value of any experiment in social psychology can be gauged, to a great extent, by the

degree to which artificiality is reduced. Successful laboratory experiments in social psychology have partly met this problem by disguising the real object of the study. This was a technique that seemed to recommend itself especially to the study of propaganda. Accordingly, a laboratory experiment was designed to investigate the influence of propaganda on performance on a skilled task and was conducted in such a way that the learning of the propaganda would take place *incidentally* in order that the general character of the response might approximate as far as possible to the real life situation.

In this experiment, 'propaganda' was left next to the instructions for a task, and its influence on the subjects' subsequent performance was measured in relation to the time they spent attending to it, how much they understood it and how well they could describe or recognize it.

(ii) *Experimental procedure*

(*a*) *Design of experiment.* Fifty-eight subjects (naval ratings, university students and 14-year-old schoolchildren) were given a mechanical task. Their aim was to score as high as possible in a given time, choosing one of two speeds. The printed instructions pointed out the relative advantages of using each speed. Experience on the task soon revealed that higher scores could be made at the slow speed. However, a propaganda display placed next to the instructions suggested that better results were gained by choosing the fast speed. The experiment was conducted under two conditions: Group A: propaganda was displayed while the subjects were learning the task; Group B: propaganda was displayed when the subjects had learned the task and had thus formed their own opinion of which speed produced the highest scores.

(*b*) *The task.* A 'triple-tester' was used. The speed of the rotating drum on which the 'dot targets' appeared could be adjusted by the experimenter. Both speeds were demonstrated and the subjects were able to choose before each of five half-minute runs which one they preferred. The scores were not visible to the subject. After the fifth run they were asked to choose a speed for a longer run but were not actually given the task. Group B returned the following day and, after revising the instructions (beside which the propaganda was now displayed), they were told to choose the fast or slow speed for a longer run. Thus, their choice on the second day (which might be affected by the propaganda) could be measured against the choice they had recorded on the previous day.

(*c*) *The propaganda.* The implication of the propaganda was that the highest scores on the task had been made by those who had chosen the fast speed. A coloured histogram chart showed the scores of previous subjects who had chosen the fast speed in relation to those who had chosen the slow.

(*d*) *Methods of measuring propaganda 'effectiveness'. Attention*: Subjects were allowed to read the instructions for as long as they wished. From the other side of the room, the experimenter was able to measure, with a silent stop-watch, how much of this time had been devoted to attending to the adjacent propaganda. *Behaviour*: The choice of fast or slow speed for each round was noted. For group B, the speed which they preferred for the longer-run, both before and after seeing the propaganda, was recorded. *Recall of propaganda*: Subjects were asked to draw and to describe as much as they could of the diagram which had been next to the instructions. *Understanding of propaganda*: In order to assess whether subjects had understood the implication of the propaganda as a whole, a form of reconstruction test was given. Subjects had to match four sets of five state-

ments each and then to fit a blank card (which represented the propaganda) into one of these sets. The test, which is described fully elsewhere (Belbin, 1953), attempted to measure understanding of the propaganda without involving verbalization. *Recognition of the propaganda*: Each subject was given a recognition test and asked to identify the 'diagram next to the instructions'.

(iii) *Results*

Attention to propaganda. There was a striking contrast between the two groups both in the numbers who attended to the propaganda and in the amount of time they spent in attending to it.

In group A, seven subjects attended for

$$20, 10, 9\tfrac{1}{2}, 7\tfrac{1}{2}, 5, 2, 1\tfrac{1}{2} = 55\tfrac{1}{2} \text{ sec. (mean} = 7{\cdot}9 \text{ sec.)}$$

In group B, twenty subjects attended for

59, 50, 47, 40, 38·2, 29, 28·5, 28·1, 21, 20, 15, 12·5, 5·5, 5, 4·5, 4, 3·5, 2, 1, 0·5 = 414·3 sec. (mean = 20·7 sec.)

Of these twenty-seven subjects who attended, thirteen understood it, fourteen did not. The amount of time spent in attending varied from 2 to 59 sec. for those who did understand it, and from 0·5 to 50 sec. for those who did not.

Recall. Of the forty-one subjects who could describe the propaganda in full or part detail, thirteen understood it and twenty-eight did not. Four subjects, with recall scores of 5, 5, 4 and 3 out of a possible 6, gave a converse interpretation of it, i.e. that it was better to choose slow than fast.

On the other hand, some subjects who had understood the propaganda were not able to describe it at all. One subject, for example, who had obviously understood the propaganda was only able to recall 'a graph with something red on it'. He was quite convinced that the implication of the graph was that a fast speed was better than slow, although he was quite unable to describe how he gained that impression. The recall scores of those who did understand the implication of the propaganda varied from 1·25 to 6 points.

Recognition. Table 4 shows the number of subjects who were successful in the recognition test compared with those who attended to the propaganda. It can be seen that nine subjects, who had attended to the propaganda, said they did not recognize it. As all these nine subjects had been able to recall at least some of the propaganda, recognition may

Table 4. *Comparison of those who attended to the propaganda with those who recognized it*

	No. who attended	No. who did not attend
Correct identification	18	19
Incorrect identification	9	12

have been adversely affected by the interpolated recall (Belbin, 1950*b*). However, nineteen of the subjects, who had not 'attended' to the propaganda at all, said they recognized it. These nineteen could only have seen the propaganda peripherally whilst engaged on the task of reading the instructions. It would have been impossible for them to have understood its content or to have drawn any conclusions from it since the detail was too great

to be covered by a single span of apprehension. Of the thirty-seven subjects who correctly identified the propaganda in the recognition test, only eight understood it.

Behavioural change. Of the eighteen subjects who originally chose slow for the long run, four changed to fast after seeing the propaganda. None of the eleven subjects who originally chose fast for the long run changed the speed after seeing the propaganda. One of the four who changed to fast had not understood and could not, therefore, have been influenced by the propaganda. It is interesting to note that the other three (all of whom admitted that their change of speed had been made because of the propaganda) were, in fact, reverting to their original choice of speed for the first five runs before experience on the task had changed their decision. The propaganda was in fact serving to reinforce an original decision.

Effect of propaganda under the two different conditions. In the learning situation (group A): seven subjects attended to the propaganda, of whom two understood it, of whom two chose the fast rate. When the task had been learned (group B): twenty subjects attended to the propaganda, of whom eleven understood it, of whom five chose the fast rate, of whom three had been influenced by the propaganda (one had originally chosen fast, one had not understood the propaganda).

Table 5 shows the test results of the subjects whose behaviour was influenced by the propaganda.

Table 5. *Test results of subjects whose behaviour was influenced by the propaganda*

	Attention time in seconds	Items recalled (total 6)	Whether propaganda was correctly identified in recognition test
Group A	9·5	4·0	No
	20·0	6·0	Yes
Group B	12·5	5·5	No
	28·1	6·0	No
	38·2	3·5	No

It can be seen from this table that neither duration of attention nor number of points recalled were any guide to the ultimate effect of propaganda on behaviour. The results of the recognition test suggest that non-recognition was a better guide to effectiveness! If a recognition test had been used as the criterion of effectiveness thirty-seven subjects would have been successful.

IV. Discussion

It is clear from these results that the effect of the propaganda varied according to whether the subjects had had previous experience on the task and according to whether its influence was assessed by its attention value, its intelligibility, or by its capacity to influence behaviour.

Some subjects had only attended to individual items of the display and, although they were able to recall or recognize these, they were not able to understand the propaganda and were not influenced by it. Other subjects understood and recalled or recognized the propaganda but were not influenced by it. Finally, there were those subjects who understood the propaganda and were influenced by it but who could not remember it. Thus it seems that the information which had been learned from the propaganda material could

become apparent in two distinct forms: (*a*) in recall or recognition, or (*b*) in subsequent behaviour. It was not clear, however, what caused effect (*a*) or (*b*) to be more marked in some subjects than in others.

One of the main disadvantages of using a recognition test was its inability to predict *what* a subject had actually perceived in the display. In many cases, recognition seemed to be based on memory for visual form rather than for meaningful content. Vernon (1945) has shown how individuals sometimes clearly visualize graphical material without translating the images into the information the graph was intended to convey. It is thus hardly surprising that responses in recognition tests and those in tests of 'behaviour' are so little related.

Similar factors could not explain, however, why some subjects were not influenced by the propaganda although they could recall and understand it. Nor, from a study of the literature, is there a comprehensive theory upon which all our results could be founded. Bergson 1913, and later McDougall (1923), distinguished between two ways in which the past '. . . appears to be stored up', postulating motor mechanisms making use of the past in action, and 'pure' memory being the ability to call up the image of an experience. The term 'use of the past in action', while at first sight appearing to resemble the response of our subjects who used the information, was actually envisaged by Bergson as comparable to habit formation expressed, for example, in verbal repetition habits where the memorization was exclusively dependent upon specific motor mechanisms. It is thus hardly comparable to our groups who could not verbalize but who were able to extract the information from the propaganda for use in another task. A different theoretical assumption has been made by Katona (1940) and Szekely (1950*a*, *b*), who demonstrated how information could either be recalled or used in relation to problem solving or mechanical tasks respectively. They suggest that the difference which exists is not solely one of memory function but is due to different learning processes being involved at the outset. The extent to which disparity between the tests of recall and tests of actual behaviour may be interpreted in terms such as these cannot at present be estimated. It will first be necessary to consider in more detail the conditions which determine a response to propaganda. These will be examined in the second part of this paper.

REFERENCES

BELBIN, E. (1950*a*). Report on Road Safety Propaganda in Children's Cinema Clubs. D.S.I.R. Road Research Laboratory. Note No. RN/1300/EB.

BELBIN, E. (1950*b*). The influence of interpolated recall upon recognition. *Quart. J. Exp. Psychol.* **2**, 163–9.

BELBIN, E. (1953). The effect of propaganda on recall, recognition and behaviour. Cambridge University Library, unpublished Ph.D. Thesis.

BERGSON, H. (1913). *Matter and Memory.* London: Allen and Unwin.

CHEN, W. K. C. (1933). The influence of oral propaganda material upon students' attitudes. *Arch. Psychol.* **23**, no. 150.

ECKSTRAND, G. & GILLILAND, A. R. (1948). Psychogalvanic method for measuring the effectiveness of advertising. *J. Appl. Psychol.* **32**, 415–25.

GALLUP, G. (1950). Impact—a new method of evaluating advertising. *Journalism Quart.* **27**, 378–82.

GARDINER, I. C. (1935). Effect of a group of social stimuli upon attitudes. *J. Educ. Psychol.* **26**, 471–9.

HANFMANN, E. (1941). A study of personal patterns in an intellectual performance. *Char. & Pers.* **9**, 315–25.

JARRETT, R. F. & SHERRIFFS, A. C. (1953). Propaganda debate and impartial presentation as determiners of attitude change. *J. Abnorm. (Soc.) Psychol.* **48**, 33–41.

Jenkins, J. G. (1938). Dependability of psychological brand barometer. I. The problem of validity. *J. Appl. Psychol.* **22**, 252–60.

Katona, G. (1940). *Organizing and Memorizing.* New York: Columbia University Press.

La Piere, R. T. (1934). Attitudes *v.* Actions. *Social Forces*, **13**, 230–7. Reported in Doob, L. W. *Public Opinion and Propaganda*, 1948. Holt: New York.

Lazarus, R. S. & McCleary, R. A. (1951). Autonomic discrimination without awareness. *Psychol. Rev.* **58**, 113–22.

Link, H. C. (1934). A new method for testing advertising and a psychological sales barometer. *J. Appl. Psychol.* **18**, 1–26.

Longstaff, H. P. & Laybourn, O. P. (1949). What do readership studies really prove? *J. Appl. Psychol.* **33**, 585–93.

MacCurdy, J. T. (1928). *Common Principles in Psychology and Physiology.* Cambridge University Press.

McCreary, J. R. (1953). The Modifications of International Attitudes. *Victoria Univ. College Publ. in Psychol.*, no. 2. New Zealand.

McDougall, W. (1923). *Outline of Psychology.* London: Methuen.

Miller, J. G. (1939). Discrimination without awareness. *Amer. J. Psychol.* **52**, 562–78.

Poffenberger, A. T. (1932). *Psychology in Advertising.* New York: McGraw Hill.

Rubin-Rabson, G. (1941). Studies in the psychology of memorizing piano music. VI. A comparison of two forms of mental rehearsal and keyboard overlearning. *J. Educ. Psychol.* **32**, 593–601.

Smith, F. T. (1943). *An Experiment in Modifying Attitudes towards the Negro.* New York: Harlem Studies. Reported in Murphy, Murphy & Newcomb, *Experimental Social Psychology.* New York: Harper.

Stern, W. (1938). *General Psychology.* New York: MacMillan.

Szekely, L. (1950*a*). Knowledge and thinking. *Acta psychol., Hague*, **7**, 1–24.

Szekely, L. (1950*b*). Productive processes in learning and thinking. *Acta psychol., Hague*, **7**, 388–407.

Vernon, M. D. (1945). Learning from graphical material. *Brit. J. Psychol.* **36**, 145–57.

Welch, A. C. (1941). Testing competitive advertising. *J. Appl. Psychol.* **25**, 176–90.

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A STUDY OF CHILDREN'S INTERESTS AND COMPREHENSION AT A SCIENCE MUSEUM

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As part of an attempt to increase the attractiveness and usefulness of a Children's Gallery in a Science Museum, an inquiry was made into the numbers and types of children who visit it, their behaviour there, and the extent to which they comprehend what they have seen. Methods of investigation included a sampling survey of attendances at different times of day and periods of the year, stop-watch recording of times spent at different exhibits and charts of movements, and an oral questionnaire dealing with the background of one hundred children and their understanding of selected exhibits. The data throw some light on the evolution of genuine scientific interests (mainly in boys) from superficial curiosity, and on suitable and unsuitable exhibits and methods of display and labelling.

I. Introduction

Every year some 300,000 children visit the Science Museum, South Kensington, in addition to some 70,000 who come in organized school parties. In 1952, the advice of the senior author was sought on how to increase the attractiveness and usefulness of the Museum to such children, and an investigation was then undertaken by the junior author in an attempt to elicit some of the psychological factors involved. This study was confined in the first instance to the Children's Gallery—a large basement room containing some thirty groups of varied exhibits designed to be interesting and comprehensible to children. The Gallery aims both to give children a simple introduction to the theme of Science in Daily Life, and to provide a place which will attract them sufficiently to reduce the amount of disturbance they might otherwise cause to more serious students in the main Galleries.

Though many efforts have been made to develop Museum services for children (notably at the Geffrye Museum, the Natural History Museum, at Haslemere and at Glasgow, as well as abroad), little scientifically controlled research has so far been reported. In one study at the Cambridge Museum of Archaeology and Ethnology, A. S. Wittlin (1949) demonstrated the greater effectiveness of a carefully structured exhibit over an old-fashioned hotch-potch display, by conducting an informal oral questionnaire with visitors. Very relevant is the large amount of American work on readability of educational materials, together with studies by B.B.C. staff and by the senior author (Vernon, 1951) of the intelligibility of educational broadcasts. These have demonstrated the extent to which children and adults of average or poor education fail to comprehend printed matter or talks, even when specially designed for them. The main hindrances included difficulties of vocabulary and sentence structure, abstract or uninteresting content, poor organization or structure, and absence in the readers or listeners of technical background necessary to an understanding of scientific, economic or similar subjects. A cursory inspection of the labels in the Children's and other Museum Galleries suggests that many of these might likewise fail in their purpose of explaining the exhibits, except to persons with some scientific education.

Three main techniques of investigation were applied, in addition to informal observation and questioning:

(1) A sampling survey of the numbers, ages, etc., of visitors to the Children's Gallery at different times of day and periods of the year.

(2) Unobtrusive recording with stop-watch of the behaviour of fifty children in the Gallery.

(3) Application of a systematic oral questionnaire to 100 children at the end of their visits.

II. Attendances

The numbers of persons entering the Children's Gallery were counted during six quarter-hour periods on two days falling under each of the following headings:

(*a*) Summer term weekday.
(*b*) Autumn term weekday.
(*c*) Summer holiday.
(*d*) Christmas holiday
(*e*) Bank Holiday.
(*f*) Fine Saturday.
(*g*) Wet Saturday.
(*h*) Sunday afternoon.

The visitors were classified by sex and—approximately—by age into: adult, 13–16 years, 6–12 years, less than 6 years. Comparisons of the numbers of the two sets of forty-eight occasions gave an index of the reliability of the sampling. The distributions were mostly skewed (i.e. many small attendances, few large ones), and it was considered that tetrachoric correlations, corrected by the Spearman–Brown formula, would provide the most appropriate, even if rough, reliability coefficients. These coefficients are shown along the diagonal in Table 1. In addition, the figures for each type of person in the two sample periods were combined and intercorrelated, to indicate the extent to which high attendances by one type go with high attendances by other types.

Table 1. *Reliabilities and intercorrelations of numbers of adult and child visitors on different occasions*

	Men	Women	13–16 boys	6–12 boys	0–5 boys	13–16 girls	6–12 girls	0–5 girls
Men	**0·95**	0·69	0·55	0·85	0·85	0·68	0·83	0·57
Women	0·69	**0·89**	0·34	0·86	0·81	0·79	0·82	0·84
13–16 boys	0·55	0·34	**0·45**	0·62	0·48	0·54	0·52	0·34
6–12 boys	0·85	0·86	0·62	**0·97**	0·77	0·68	0·87	0·61
0–5 boys	0·85	0·81	0·48	0·77	**0·98**	0·26	0·56	0·60
13–16 girls	0·68	0·79	0·54	0·68	0·26	**0·76**	0·84	0·70
6–12 girls	0·83	0·82	0·52	0·87	0·56	0·84	**0·89**	0·76
0–5 girls	0·57	0·84	0·34	0·61	0·60	0·70	0·76	**0·90**

The mean reliability of 0·85 indicates that the two samples provide a reasonably stable count of the numbers at these times of day and periods of the year. The intercorrelations suggest:

(*a*) The presence of a strong 'general factor'; on any occasion when any one group shows a high attendance, other groups tend to be large also.

(*b*) That older boys are much the most independent from other types, as well as being the most irregular in their attendances.

(*c*) That 6–12-year children chiefly tend to come with adults or in groups together, and children under 6 with an adult or an elder child of the same sex. The statistical data, however, only show that the attendance habits of these groups are similar; and further direct observation would be needed to show how parties of visitors are commonly made up.

It was decided to group the visitors under four main types and to graph their attendances by time of day on each of the 8 days. Table 2 lists the numbers recorded per two for quarter-hour periods around the given times. From these figures, totals were estimated each type of day. It was desired to get a picture of the average attendance at different

Table 2. *Attendances in two quarter-hour periods on different types of day*

A. = men and women; B = 6–16-year boys; G = 6–16-year girls; C = children under 6.

Time	People	(*a*) Summer term	(*b*) Autumn term	(*c*) Summer holiday	(*d*) Autumn holiday	(*e*) Bank holiday	(*f*) Fine Saturday	(*g*) Wet Saturday	(*h*) Sunday afternoon	
10.15 a.m.	A	0	14	21	15	29	24	32	—	
	B	2	4	32	34	8	22	21	—	
	G	0	0	7	1	7	4	5	—	
	C	0	0	8	3	4	2	8	—	
11.30 a.m.	A	27	12	54	15	127	26	89	—	
	B	21	2	41	36	36	13	39	—	
	G	8	3	13	5	33	3	18	—	
	C	0	4	8	3	28	12	14	—	
1.30 p.m.	A	24	18	67	23	116	16	58	—	
	B	19	1	50	22	55	15	31	—	
	G	7	6	18	2	38	3	14	—	
	C	4	2	9	3	32	1	8	—	
3.30 p.m.	A	43	30	110	25	249	53	139	182	
	B	18	12	132	44	70	32	59	81	
	G	8	0	46	13	67	16	26	33	
	C	3	3	10	3	48	16	31	41	
4.30 p.m.	A	42	32	111	12	284	131	119	200	
	B	27	6	78	4	65	59	66	52	
	G	8	0	27	4	53	11	29	48	
	C	3	3	17	8	53	24	28	40	
5.30 p.m.	A	14	6	42	3	183	18	68	110	
	B	5	1	18	6	44	7	21	44	
	G	3	0	13	0	27	3	23	29	Whole
	C	4	0	6	0	30	5	6	22	year
Whole-day estimates	A	435	310	1140	315	2640	710	1400	1210	249,000
	B	275	70	1000	420	770	395	660	440	133,500
	G	100	30	350	75	640	110	310	260	54,000
	C	40	40	160	60	540	165	260	260	41,500
All persons		850	450	2650	870	4590	1380	2630	2170	478,000
Multiplying factor		80	110	40	25	3	26	26	53	—
Ratio BGC/A		0·95	0·45	1·32	1·76	0·74	0·94	0·88	0·79	0·92

times on a typical or average day, and to check the overall figures by comparing them with the Museum's total. Most of the 300,000 children coming annually probably enter the Children's Gallery, though a minority may go to the Main Galleries only. With this purpose, the year was somewhat arbitrarily split into 80 days of type (*a*), 110 of type (*b*), and so on; winter and early spring days were judged equivalent to autumn ones. These numbers are referred to in Table 2 as the multiplying factor. Though there are obviously several dubious assumptions in these calculations, the grand total of 229,000 children per year is just about what might be expected.

Fig. 1 gives a smoothed graph of numbers of visitors on a hypothetical typical weekday.

The most striking features of Table 2 and Fig. 1 are the enormous variations on different types of day, and the finding that adult visitors normally exceed children of all ages. On most days attendances rise to a fairly stable period from about 11.30 to 2.0, and then

climb to a peak around 4.0. The highest attendances are on Bank Holidays and Sunday afternoons. At peak periods over 200 enter the Gallery per quarter hour, and there is considerable overcrowding.

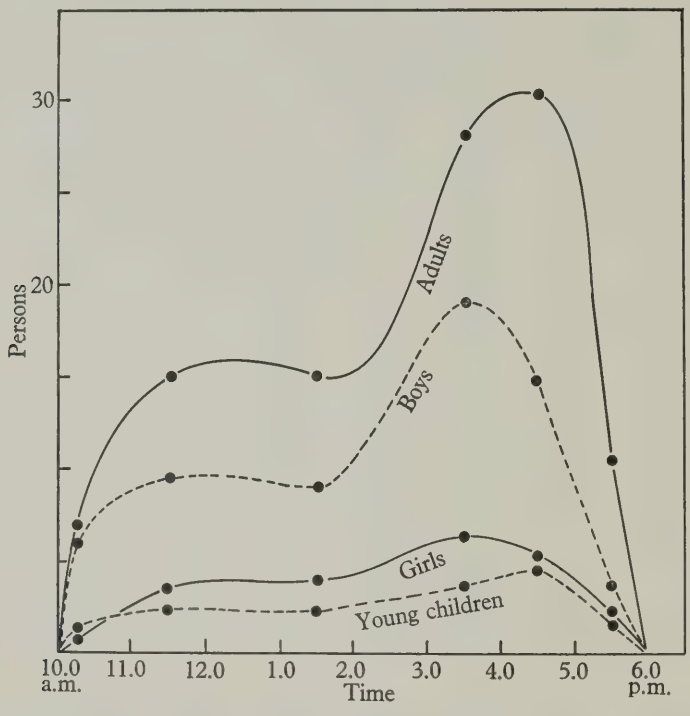

Fig. 1. Visitors entering the gallery per quarter-hour on an average weekday.

The last line of the table shows that children are in a majority only during holiday weekdays. Many of the adults come in order to bring children, and doubtless serve a valuable function in explaining the exhibits to them. But an appreciable proportion come on their own, possibly because they are wandering round the Museum aimlessly, or possibly because their level of appreciation and understanding is often similar to that of the children for whom the Gallery was designed.

III. Records of children's behaviour

A plan of the Gallery was prepared, and on this were recorded the times spent by individual children or pairs at each exhibit. Charts were thus obtained for thirty-five boys and fifteen girls who came unaccompanied by adults. This method was suggested by Marston's (1925) studies of children, where he related types of behaviour in a museum to assessments of their extraversion-introversion. Though our sample is small, it should yield some indication of the attention-catching value of each exhibit. Table 3 lists the exhibits in order of total child × time spent. It also gives the numbers who looked at each exhibit even for a second or two, and the numbers (out of 100) who—in a later inquiry—mentioned each exhibit as their favourite.

It may be seen that less than half the exhibits are looked at by 50% or more of the

Table 3. *Time spent, numbers viewing and numbers choosing each exhibit as favourite*

D = dioramas or models; A = apparatus or specimens; W = working models, etc.

Exhibits	Total min. spent by 50 children	Nos./50 looking	Favourite nos./100
Communications over long distances (D, A, W)	132·13	43	12
Methods of lifting, pulleys, jacks (W)	84·53	39	1
Experiments in a vacuum (A, W)	65·69	26	8
Stage-lighting (D, W)	54·38	31	2
Transport through the ages (D)	49·16	29	19
Photoelectric door (W)	48·90	39	12
Forces in a bridge when engine crosses (D, W)	39·19	36	—
Photoelectric burglar alarm (W)	35·46	37	9
Lighting through the ages (D)	33·84	32	9
Eclipse of the sun (W)	24·83	25	2
Sources of power, Archimedes screw, etc. (D, W)	23·73	26	4
Measurement of time (A, W)	21·99	19	2
Photoelectric sorting of objects by colour (W)	17·26	22	—
World clock (W)	14·08	28	—
Photoelectric parcel counting (W)	11·88	21	3
Magnification by lenses (A)	10·56	18	—
Making tools, hand nails, flints (D, A)	10·43	19	3
Street lighting (D)	7·76	16	5
Photoelectric control of street lamps (W)	7·65	17	—
Expansion of heated metals (W)	7·31	4	1
Mining (D)	7·07	21	—
Methods of transport, ships, carriages (D)	4·02	15	—
Old and modern kitchens (D)	4·00	15	1
Roman pottery kiln (D)	1·71	16	—
Roman villa (D)	0·15	3	—
Illuminated photographs, various	0·05	4	—
Manufacture of pottery (A)	0·00	0	—
Pressing buttons, turning handles, seeing how things work			7
Total			100

children, and that less than half are studied for more than 1 min. by the average child who looks at them at all. This is much what one would expect of any museum visiting. However, these average figures conceal a wide range of variation. A proportion of children do spend 5–10 min. or more at particular exhibits that arouse their interest. Almost always these are of the working model rather than the static diorama type. But a larger proportion tend to flit from one thing to another, stopping to press buttons or turn handles, and treating the Gallery more as an amusement arcade than as a source of scientific information.

The average time spent in the Gallery by boys was 22·4 min. (ranging up to 45 min.), and by girls 14·6 min. This weaker interest among girls is borne out by our finding that only 40% as many girls as boys come at all.

IV. Oral questionnaires

The investigator buttonholed sixty-eight boys and thirty-two girls (unaccompanied by adults) as they were leaving, and asked them to answer some questions in a quiet adjoining part of the main Galleries. No precise scheme of sampling could be devised, but as far as can be judged these children, picked at random, are not unrepresentative of all 7–16-year visitors. Table 4 gives the age and school distribution.

Clearly the greatest proportion consist of secondary modern and older primary boys, and grammar-school girls, whose scientific interests and education possibly resemble those of younger boys. Since it is hardly possible to envisage exhibits which would be

suitable for children of all ages and all educational levels, it is probable that 11–14-year boys, who do not get specialist science teaching at school, should be regarded as the 'target', or at least as the modal, audience. Grammar-school boys often indicated that the exhibits were too childish for their tastes.

Table 4. *Ages of sample and schools attended*

Age (years)	7–8	9	10	11	12	13	14	15	16
Nos.	3	6	9	10	24	20	15	11	2

Schools	Boys	Girls
Junior	12	6
Preparatory (7–11)	5	1
Preparatory and Public (11–12+)	7	2
Secondary Modern	23	7
Secondary Technical	8	0
Grammar or High School	13	16
Total	68	32

Nineteen were visitors to London from various parts of the United Kingdom or abroad, and twenty-four came from Kensington or adjoining boroughs—nine of these on foot or by bicycle. The remainder—more than half—came from more distant parts of London, as far as Croydon, Hendon, Chingford and Woolwich, by means of buses, tubes or both. Their average return fare would amount to about 1*s*. Although we have commented on the superficiality of the scientific interests of many children, the trouble and expense they incur in getting to the Museum are noteworthy. They are not merely local residents visiting a fun fair. There is little else in the neighbourhood, except other museums, to attract children.

In answer to questions about their first visit:

28 were brought by parents or relatives
14 by friends
5 by brothers or sisters
15 in school, Scout or Guide, parties
35 were told about it by parents, friends or school
3 found it for themselves.

(Only nine of the two latter groups were girls.) The median age claimed for first visit was 10–11 years, and the great majority came at between 7 and 13 years. The numbers of visits claimed (obviously a rather unreliable datum) were:

Visits	Boys	Girls
1	14	15
2	15	8
3–4	10	6
5–9	15	2
10–19	8	1
20–40	6	0

It would appear again that the majority of boys are habitués who, once they have been brought or had their interest aroused by an adult or friends, return, sometimes with considerable frequency. Girls indulge in repeated visits much less frequently than boys. When asked why such visits were made, 219 reasons were given, which could be classified under the following headings:

Helps school work	66
Mechanical and scientific interest	82
Amusement	69
Somewhere to spend the day	2

In spite of the unreliability of children's introspections, this suggests that a majority do possess at least an incipient serious purpose in coming.

After asking children to name their favourite exhibit (cf. Table 3), they were given a choice of three out of five and told to explain 'how they work'. Answers were graded:

+2	fairly good conception
+1	some conception
0	don't know
−1	confused
−2	very confused

Inadequacy of expression and ignorance of technical terms were not considered. Though there is, of course, some subjectivity in marking, fair consistency was reached (as in the writer's study of educational broadcasts) by listing beforehand the essential points to be met by a +2 or +1 answer. Eleven children (9–13 years) could not say anything about any of the five exhibits, and several others attempted only two exhibits; hence the total answers are 212 instead of 300.

Table 5. *Results of oral comprehension test*

Exhibit	Marks +2	+1	0	−1	−2	Total answers	Total +answers	Mean score for those attempting
Photoelectric door	3	38	16	4	7	68	41	+0·38
Forces (engine)	7	17	6	4	18	52	24	−0·17
Pulleys	11	5	15	3	10	44	16	+0·09
Burglar alarm	11	8	8	2	1	30	19	+0·87
Archimedes screw	3	6	6	2	1	18	9	+0·44
Total	35	74	51	15	37	212	109	

The burglar alarm seems to be the most comprehensible, to those who attempt it. The photoelectric door and Archimedes screw also get a majority of plus answers, though very few children have a good grasp of how they work. The pulleys, though one of the most patronized exhibits, probably illustrate too difficult a principle. They and the engine have particularly complex labels. Some resemblance may be noted between these results and the votes for favourite exhibits (Table 3), suggesting that children do not particularly like exhibits which they do not understand, merely because they can play about with them actively.

Though these results are limited to a few exhibits, they show that the oral comprehension test is a feasible technique for evaluating both the exhibits and the amount that children gain. Unsolicited remarks were useful also. Thus there was frequent mention of the length and complexity of some of the labels, and many children suggested that they would like somebody to be there to explain things to them.

V. DISCUSSION

The children who visit the Museum on their own, or in family groups (as distinct from school parties), vary enormously in age, intelligence and scientific education and interests. However, it would appear that many 11–14-year-old boys, even if they have little or no science in school, are sufficiently curious about the applications of physical science to justify a Gallery like the present one, in which a series of simple exhibits are provided to illustrate basic principles and techniques and the broad lines of technological

development. The main Galleries can, and do, contain valuable material which can be related to school science courses. But the Children's Gallery should not be tied to any syllabus; rather it should appeal to their sense of wonder at natural phenomena and mechanical achievements, to their desire to know how things work, and to manipulate things themselves. The children who come already thoroughly enjoy themselves, and many more would do likewise if there were more publicity, more commodious conditions (such as are envisaged in the near future), and if more of the exhibits were more readily intelligible. At the moment the activities of a large number, particularly of younger children, are very superficial and unrelated to science. Yet they tend to return many times, and it is reasonable to assume that the button-pressing and gaping do often represent the early stages of evolution of a genuine interest. Moreover, they do usually want to understand, even if they lack the perseverance or the reading capacity or educational background to make proper use of the opportunities offered. We have said little about girls, but our evidence clearly suggests that fewer of them than of boys evince the same kind of scientific curiosity, and then not until a distinctly later age.

In spite of the small samples and other inadequacies of the data, some light has been thrown by the present inquiry on the most profitable kinds of exhibit. In assessing these it is desirable to take into account not only time spent, ratings for popularity and level of comprehensions, but also the distribution of responses. For example, expansion of heated metals is low on our list (Table 3), but is surely a worth-while exhibit in that the few children who looked at it usually spent as much as 2 min. doing so. Working models clearly have the greatest pull, though some dioramas closely related to daily life interests (e.g. transport and lighting) are often high in popularity and time spent. Others, however, are apt to appear antiquated or grotesque. Probably the bottom quarter of exhibits on our list could be eliminated without appreciable loss, though it would be desirable to confirm this by more extensive observations. Obviously the range of exhibits which children can be allowed to handle or manipulate is limited; but the glass cases enclosing most of them do tend to detract from the sense of reality, and this 'behind-glass' feeling might be reduced by more use of curved glass panels.

Finally, we would stress that the planning of a museum to be of value to children is intimately bound up with child psychology. It should be based on knowledge that the psychologist could contribute of the origins and development of their interests and of the range of their capacities, rather than on subjective adult judgements. Yet at the moment the psychologist can give little detailed advice on these points, and there is very great scope for further research—research which would be welcomed by most museum authorities.

References

Marston, L. R. (1925). The emotions of young children. *Univ. Ia Stud. Child Welf.* no. 3.

Vernon, P. E. (1951). The intelligibility of educational broadcasts. *B.B.C. Quarterly*, **5**, 4.

Wittlin, A. S. (1949). *The Museum: Its History and Tasks in Education.* London: Routledge and Kegan Paul.

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'ABNORMAL BEHAVIOUR' INDUCED BY NITROUS OXIDE

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Psychological changes induced in normal volunteers by inhalations of nitrous oxide are described. Some of these changes resembled psychiatric symptoms, for example illusions and hallucinations, perseverations, and 'dissociated' states.

There was no relation between individual differences in the number of changes reported, ratings for emotional stability, and cognitive impairment attributable to the drug.

Possible relations between normal, drug-induced, and other forms of abnormal behaviour are discussed.

I. Introduction

It has long been known that certain drugs which act on the central nervous system can temporarily induce a variety of unusual behaviour phenomena in normal subjects. The resemblance between such phenomena and those characteristic of psychiatric disturbances was already noticed by Kraepelin (1892), who thought that the study of drugs might therefore throw light on the nature and etiology of mental illness.

Among more recent exponents of a similar approach have been Lindemann & Malamud (1934) working with sodium amytal, cocaine, hashish and mescaline, Lindemann (1935) with adrenaline, Newman (1935) with alcohol, Levin (1946) with bromides, Bromberg (1934) with marihuana, Stockings (1940), Klüver (1942), Hoch (1951) and McKellar (1954) with mescaline, DeShon, Rinkel & Solomon (1952), Hoch, Cattell & Pennes (1952*a*), Elkes, Elkes & Bradley (1954), and Sloane & Lovett Doust (1954) with lysergic acid, and Hoffer, Osmond & Smythies (1954) with adrenochrome. Various aspects of the possible relation between such 'synthetic' behaviour abnormalities and psychopathology have been discussed by Mayer-Gross (1951), Osmond & Smythies (1952), and Hoch, Cattell & Pennes (1952*b*). Until recently, however, there has been relatively little experimental follow-up in the laboratory. Most of the published studies have been qualitative and clinical, and the subjects often confined to psychiatric patients and few in number. Few investigators have given systematic descriptions and classifications of the behaviour phenomena induced, or have made statistical analyses of the frequencies with which they occur and of their relations with other aspects of behaviour.

The data to be presented here were gathered in the course of experiments (Steinberg, 1954) with the anaesthetic drug nitrous oxide (N_2O). The precise mechanisms through which anaesthetics act are imperfectly understood; available evidence has recently been reviewed by Butler (1950). Though familiar as an adjunct to psychotherapy, nitrous oxide does not appear to have been extensively considered hitherto for the purpose of experimentally inducing behaviour abnormalities as discussed above. Numerous clinical and incidental observations of its effects on behaviour have, however, been reported, for example by James (1905), Zador (1928), McKinney (1932) and Marshall (1937, 1938).

The aim of the present paper is to describe effects of nitrous oxide on a reasonably large sample of normal subjects, to classify these effects, examine their respective incidence and

finally, to explore the relation between them and other aspects of the subjects' behaviour. A fuller description of the investigation is contained in a thesis by the author (Steinberg, 1953).

II. Method and procedure

Data were obtained in the course of two series of experiments:

(1) *Main experiments*

Fifty student subjects, 36 male and 14 female, mean age 21·5 years, S.D. 3·4 years, carried out a series of timed cognitive performance tests, lasting about half an hour, first for practice and then while inhaling through a face mask a scented mixture of 30% nitrous oxide and oxygen. A further 50 subjects, matched for age and sex, served as controls; their treatment differed from that of the experimental subjects only in that they inhaled scented air instead of nitrous oxide. The experimenter: (i) recorded any incidental behaviour, including verbal behaviour, in shorthand; (ii) obtained a written introspective account as soon as the inhalation was over; and then (iii) presented orally to each subject a prepared check-list mentioning some sixteen psychological and physiological symptoms commonly encountered with this and similar drugs. Finally each subject was asked to state what he thought he had been inhaling.

(2) *Miscellaneous additional experiments*

Seven subjects attempted, but did not complete, the experimental procedure in the main experiments, owing to loss of consciousness or other unfavourable reaction to the drug. A further 12 subjects were given varying experimental treatments, including higher concentrations of nitrous oxide in oxygen. For each of these subjects the experimenter again (i) recorded incidental behaviour, and (ii) obtained written introspections; the check-list was not, however, used.

III. Results

A great variety of abnormal phenomena occurred during inhalation of the drug; except for some of the physical complaints, they usually subsided almost immediately after administration ceased.

All figures quoted refer to the main experiments and not to the miscellaneous additional experiments; illustrative material is taken from both.

(1) *Incidence of symptoms*

Table 1 shows the proportion of subjects who reported each particular kind of psychological and physiological change. The much higher incidences reported in reply to check-list questions, as compared with the spontaneous introspections, were probably partly due to the suggestion effect of the questions and partly to most subjects' lack of familiarity with the process of introspecting; possibly also the check-list questions may, by their specificity, have helped to counteract any retrograde amnesia induced by the drug. Moreover, though the control group showed analogous discrepancies between spontaneous and check-list reports, the proportion of subjects who admitted to behaviour changes in response to check-list questions was altogether very much smaller than in the experimental group, and many control subjects also qualified their replies by pointing out that

the symptoms had been very slight or only due to 'nerves'. Table 2 gives the frequencies with which some additional phenomena, not explicitly covered in the check-list, were mentioned in the introspections. It may be noted that the control group referred mainly to minor discomforts attributable to the apparatus, which seem to have been much less noticed in the experimental group.

The tables show that the frequency with which different symptoms were reported in the experimental group varied considerably. None was universally experienced by all

Table 1. *Frequency with which different kinds of symptoms were reported by the experimental and control groups*

	Percentage of subjects reporting					
	Experimental group			Control group		
Symptom	In answer to check-list $N=49$*	Introspections $N=50$	Check-list and introspections combined $N=50$	In answer to check-list $N=36$*	Introspections $N=50$	Check-list and introspections combined $N=50$
'Compulsive' laughter	12	10	20	0	0	0
Unsteadiness of finer movements	80	16	80	33	0	24
Impaired control over grosser movements	43	12	46	6	0	4
Tingling	67	30	68	6	0	4
Numbness	51	22	50	3	0	2
Visual disturbances	35	4	38	6	2	6
Auditory disturbances	49	24	52	14	6	14
'Dreaminess'	88	18	86	31	2	24
Difficulty in concentrating	92	46	92	39	18	34
Difficulty in talking	35	4	34	6	0	4
Difficulty in writing	51	10	50	8	0	6
Impaired control over whole situation	69	2	68	22	0	16
Memory disturbances	59	16	62	31	2	24
General effect of inhalation:						
Pleasant	45	12	44	28	4	22
Unpleasant	33	0	32	11	0	8
Neutral	20	2	20	56	0	40
Pleasant and unpleasant	2	0	2	0	0	0
Euphoria	69	20	72	14	6	14
Dizziness	53	10	52	14	8	12
Nature of gas:						
Drug	86	6	84	14	0	10
Air	2	0	2	69†	2	52
Both	6	4	6	0	2	2
Doubtful	6	0	6	17	6	18

* Because of shortage of time, the check-list was not given to all subjects.
† Includes five doubtful at first.

subjects, but unsteadiness of movement, tingling, numbness, dizziness, auditory disturbances, and, above all, phenomena suggestive of various aspects of personality dissociation, such as difficulty in concentrating, 'dreaminess', feelings of being 'distant' and of losing control over one's actions, appear to have been extremely common.

A 'symptom score' was computed for each subject by assigning one mark for each kind of symptom he had reported in the introspections, and/or in reply to the check-list. As is shown in Fig. 1, individual differences in the experimental group were considerable. The smallest number of symptoms reported by any one subject was 8 and the largest 22. In the control group the scores were much smaller, and there is very little overlap between the two distributions. The differences between the two groups were equally striking when the number of symptoms per subject was computed from the information given in the

introspections alone. Three of the control subjects were under the impression that they had been given a drug, and the numbers of symptoms they reported were among the largest in the control group.

(2) *Examples of behaviour abnormalities*

The examples given below have been selected partly for their intrinsic interest, and partly for their particularly suggestive resemblance to psychiatric symptoms. Four broad categories of phenomena will be distinguished, a classification which, needless to say, is by no means absolute or exhaustive. Disturbances of the perception of time, of which a large

Table 2. *Additional symptoms described in introspections*

	Percentage of subjects reporting	
Symptom	Experimental group	Control group
Discomfort from mask	18	28
Difficulty in breathing	2	26
Smell (unpleasant)	2	8
Smell (pleasant)	—	4
Sleepy, tired	12	8
Weak, faint feeling	14	—
Sick feeling	4	—
Hot, flushed	10	—
Far away, detached	36	—
Careless	38	—
More confident	6	—
Cheated in tests	8	—
'Automatic' behaviour	12	—
Some tests seemed easier than before	22	—
Performance improved in some tests	56	—
Vivid imagery	2	—
Wanted to talk, cheerful	22	—
Tendency to repeat oneself	12	—

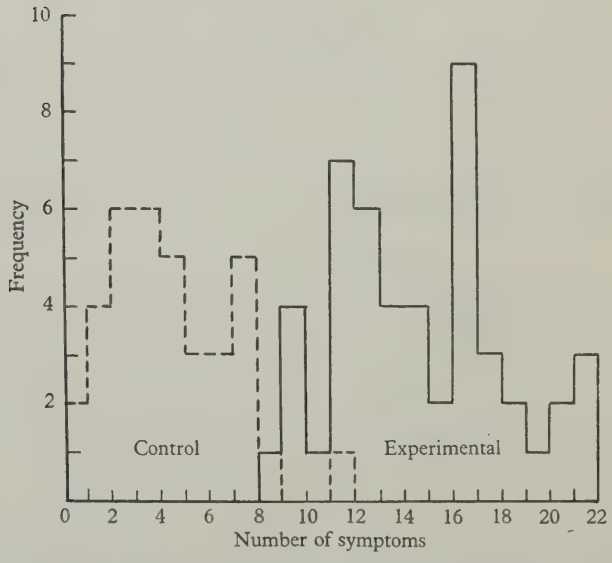

Fig. 1. Frequency distributions of the number of symptoms per subject reported in the experimental and control groups.

number were reported, have been omitted altogether, as they have been discussed in detail elsewhere (Steinberg, 1955). The concentration of nitrous oxide, if other than 30%, is given where appropriate in brackets.

(*a*) *Illusions and hallucinations*

The majority of these were auditory, possibly partly because it was found difficult to eliminate all extraneous sounds from the laboratory. The anaesthetic machine itself was almost inaudible, but a kettle of water had to be kept boiling throughout.

Music was reported by a number of subjects, though none was audible to the experimenter at any time during experiments. For example, one subject (50% N_2O) reported during a long experiment lasting 1 hr. 25 min. that a Mozart or Beethoven opera was being played 'to and fro' in an adjoining building. She referred to this altogether nine times during inhalation and on one occasion described it as 'an hallucination'. Another subject said that he 'heard a noise of what was probably a kettle boiling, but which sounded like an orchestra playing a monotonous tune'. The 'wireless' was referred to by several subjects. One reported that he heard 'a wireless with a programme on—a comedian cracking jokes and someone singing', and another remembered hearing rhythmic drumming which seemed to him to get louder and louder, 'in a manner similar to that used with effect on the films'.

Sometimes the illusions assumed what might be called a paranoid character. One of the subjects (50% N_2O), whose report is reproduced in full later, thought that the workmen on the roof of a building opposite were pointing and laughing at her. Another subject felt as if someone else far away was himself and was laughing at him. Various remarks were attributed to the experimenter which she had not in fact, made, and the content of which sometimes seemed readily explicable in terms of emotions of the moment. For example, a subject (50% N_2O) who during inhalation had been complaining of being much disturbed by the drug, suddenly removed her mask half way through the experiment saying that she took the experimenter's remark 'You can come off now', which the experimenter had not in fact made, to mean 'take off the mask now', which the experimenter had not said either. Several subjects thought the experimenter was laughing. One of them 'heard' her humming and another sighing.

One subject (50% N_2O) reported a curious visual phenomenon: on his right, where a friend of his was actually standing with his back to the wall, only a pair of legs remained; the upper half of the body had disappeared and the wall was visible behind. Similarly, the surface of the table at which the subject was sitting disappeared and only the paper and pencil remained, 'suspended in mid air'. Another subject reported that for a very short time he was unable to see, as prior to an attack of migraine to which he was prone.

One illusion involved the sense of pain. The subject reported that inhalation reminded her of the last time she took gas at the dentist's. She thought that she was hearing an alarm bell, as of an ambulance, and then felt as if she were going to have a tooth extracted. At the same time, however, she 'knew' that she was not actually at the dentist's. 'The sound of the alarm bell increased, and then died away again—at which point I felt the pain of extraction. I removed the mask because previously the pain had increased to almost unbearable intensity'. The subject actually did remove the mask 2 min. 25 sec. after beginning inhalation.

(*b*) *Irrelevant or exaggerated thoughts and emotions*

An outstanding characteristic of these phenomena was the force and persistence with which they seemed to be experienced, even though their content might be relatively trivial. Especially when higher concentrations of nitrous oxide were given the subjects' conversation often flowed very easily and irrelevantly by free association, usually taking the original cues from features of the experimental situation or their ordinary day-to-day life and occupation. For example, a zoologist talked at length about birds and insects, and students of psychology tended to express themselves in the technical language of their subject, especially in psychoanalytic terms. Sometimes the content showed more than ordinary lack of restraint.

Occasionally behaviour indicated aggressive tendencies, as when a subject reported that he felt hostile toward the experimenter, and another (40% N_2O) noisily objected to some minor extraneous noises and shook his fist so violently that he knocked a hole into an adjacent fibre-board wall. Some subjects became euphoric, and irresponsible, variously describing themselves as feeling pleased and happy, 'gay', 'less shy than usual', terribly sorry that time was going as quickly and that the experiment was over so soon'. However, overt laughter, as can be seen from Table 1, was rare, possibly because subjects were mostly kept busy with tests and were given no social stimulation. Several subjects, however, reported that they had had to suppress tendencies to laugh. One subject, who actually did laugh loudly, commented afterwards that although he knew he was laughing, he 'didn't feel facetious'—a disjunction between affective state and overt behaviour somewhat reminiscent of the well-known phenomenon of 'sham rage'. A few subjects who were heard laughing by the experimenter during the inhalation, subsequently appeared quite unaware that they had done so.

Nineteen subjects in the main experimental group of 50 commented in their introspections that they became less concerned over accuracy in the cognitive tests. Several reported that some or all of the cognitive tests seemed easier than before, although actual performance was usually found to have deteriorated. One subject thought he was 'doing particularly fine' in an arithmetic test, though in fact he had only attempted three sums and got two of them wrong. One or two tried to obtain higher test scores by cheating, for example by telling the experimenter that they had inserted a greater number of balls into a manual-dexterity test apparatus than was the case. One subject wrote in his introspections: 'In the track tracing...I found myself "cheating" quite unconsciously and carelessly.' Although, as is shown in Table 1, pleasant experiences were reported more frequently than unpleasant, a very few subjects were visibly distressed and anxious during inhalation and one said he felt 'panicky'. The male subject (35% N_2O) whose full introspections are reproduced later, experienced 'flashes of insight' and made the 'special discovery' that 'everything is as it is'.

(*c*) *Perseverations*

Occasionally, subjects interpreted environmental stimuli in a perseverative manner. For example, one reported that during inhalation (35% N_2O) he had heard the experimenter ask several times 'Are you all right; are you all right', when in fact she had only asked it once. More often, however, the subject's own actions were themselves repetitive. Thus the same subject also reported that during the experiment he kept repeating to

himself 'I am still here, I am still here . . . '. A female subject, while inhaling a rather high concentration (60% N_2O) said 'That's all right, that's all right' five times one after the other and with great emphasis. An example of perseverative motor behaviour has already been described elsewhere (Wilson, Crockett, Exton-Smith & Steinberg, 1950). A subject carrying out the manual-dexterity test under 40% nitrous oxide continued with regular and precise movements of his forceps to insert imaginary balls into the apparatus for several minutes after he had been told to stop and had exhausted his supply of balls, and only left off when the gas was withdrawn. As in the case of laughter, some subjects whose behaviour had not actually shown repetition reported afterwards that they had had to suppress tendencies to keep repeating particular words or actions.

Perhaps the most perfect example of perseveration was provided by the female subject whose introspective report is reproduced later. She was under the impression that she had become a gramophone record going round and round and round, endlessly repeating itself.

(*d*) *'Dissociated' states*

Eighteen subjects in the main experimental group and many of those in the other experiments reported feeling 'distant' in some sense. With some this involved a definite notion of another place, e.g. 'back in the Army', but more often it seemed to be a feeling of being far away, but 'nowhere in particular'. One male subject (50% N_2O) reported that 'there was a curious irrelevant appearance in my consciousness', and another (20% N_2O) said he 'felt abstracted'. In some cases these feelings seemed to involve depersonalization. The subject would appear to be watching himself, as in a dream, or parts of his body would seem misplaced or not belonging to him. For example, one subject (35% N_2O) felt far distant, and someone else, '24 years younger', seemed to be performing his tests.

(3) *Specimen verbatim introspections*

Many of the various phenomena described under the four headings above are illustrated by the two introspective reports which are reproduced below exactly as they were written by the subjects. Both subjects were studying psychology, and therefore probably more adept than average at describing their experiences. For purposes of comparison, the introspections of one control subject who was under the impression that he had been given nitrous oxide are also given.

(*a*) *Female graduate student of psychology*

The subject was given two 5 min. inhalations of 50% nitrous oxide, separated by a pause of 1 min. Her introspections were written during inhalation. As they were somewhat illegible, she transcribed them after the experiment, and added some explanatory comments which are given in brackets below.

First inhalation

Weak feeling in fingers and toes, shoulders—Aware of fast heart beat. Feel very weak, v. slow and difficult to write—Great heart beat. Think I shall collapse. Drumming and buzzing. Writing almost automatic. Tingling of lips. Drumming and buzzing. As if from a great distance (i.e. my writing looked very far away). Drumming and rhythm. (Aware of significance of rhythm as sexual symbol.) Can't help laughing. Everything much clearer but more distant than usual. Contraction across my chest. Can only think of two words ahead instead of 6–12 (Remember difficulty of estimating how many words ahead I could think) As from a great distance. Laughter and contracting from elbow to chest. From a

distance and aslant. Can't write keeping my letters together any more.* (Self criticism of 'u' for 'a'.) It does look funny. Can't see Getting quicker quicker quicker Can't help laughing laughing laughing laughing laughing feel like a gramophone record record record ('Passivity feeling'.).

Second inhalation

I still feel rather weak and numb about the extremities, rather distant, my heart is rather fast, there is a big rhythm in the machine and I can't quite control my hands. The rubber† (which touched my knees at this point) is like a big snake and there is big drumming. He is going out (The man in white‡ went out at this point and I felt sexually attracted to him!) And it is difficult to cross my i's and not to write backwards (it looked very left-sloping in the 'distant' view) which must be left-handed or neurotic and I write smaller and smaller almost automatic. (looked like writing through wrong end of binoculars or the pit in the 'Snake Pit'.) I must be getting pas. ('going to pass out') I can't help writing left handed (i.e. sloping backwards). This is my Ph.D.§ This is my Ph.D. (i.e. neurotic symptoms under gas.) it is fun repeating myself. Smaller and smaller smaller and smaller. So funny humming humming like a dance tune from Oklahoma (the one the aunt sang about 'When a fellow asks a girl—I can't say no', but I did not hear the words) Isn't it Rhythm Drumming it is so clear from a distance. Don't know if I can read it close-up. Biting myself (I think I was biting my tongue quite hard.) I can hear close up music now ('close up' was some sort of association with films) Cookhoo Cuckoo I can't spell Dance music and giggling!!

Feel so giggly though can't write properly but I can TALK! Oh how I can talk. Does this go back to early childhood? It's wearing off and my mask is askew.

(*b*) *Male student of psychology*

The subject inhaled 35% nitrous oxide for about 30 min., while carrying out the same cognitive tests as those used in the main experiments. He began writing his introspections a few minutes after the inhalation was over.

I felt as if I were far distant and that the individual performing the tests were someone else, several years younger, say twenty-four years younger. I experienced some early memories, or perhaps the affective tone accompanying past experiences. Throughout, there was a tendency to be sick, and another to go to sleep. The super ego function was intensified in such a way that an actual splitting of the mind seemed to occur, the alternate phases being devoted to 'self' and 'higher self' respectively. This latter functioned critically.

There were flashes of insight, or apparently so, and a desire to stop doing the particular test in order to follow up a special discovery. These discoveries took the form of enlightenment concerning the nature of mind. One such piece of insight was the uncovering of the truth that 'everything is as it is'. Having discovered this, I would try to pursue the implications of the proposition, only to remember that the task in hand was to complete the performance test.

Temporal relations were replaced by spatial ones. When thinking of a childhood experience I myself would at that moment be looking down from far above; if the experience were a recent, everyday one, I would, rather, be taking part in it.

A feeling of numbness persisted. Tactile sensations lost intensity and significance. The tendency to salivate was strong.

I am not convinced of the worthlessness of some of the thoughts that passed through my mind. For instance, the desire to be sick had the concomitant thought, 'This gas makes me sick.' Then I associated this with a visit to the dentist. Dentist plus gas equals sickness. But if the dentist alone had once been the cause of sickness, the experiencing of the gas which called up the image of the dentist would call up the corresponding affect of sickness. The gas might therefore be uncorrelated with the *cause* of sickness. No appreciation of this idea resulted in a diminution of my fear of being sick. As an aside, the 'image' of the dentist was not a visual one; if anything, it was olfactory; it may not have been an image at all, but a 'functional gnosia' from which the psychical concomitants were divorced.

But even what flashes of apparent understanding into 'new truths' there were, these were undoubtedly heavily weighted with a degree of significance which they didn't deserve. The idea occurred that 'a

* The subject's handwriting had deteriorated, and she was leaving letters like 'a' and 'o' partly open.

† Corrugated rubber tubing which connected the subject's face mask with the anaesthetic machine.

‡ A male technician.

§ The subject was at that time searching for a suitable research project for her Ph.D.

person's behaviour is not necessarily the effect of a certain series of events, but may be the effect of another series of events'. Stated in this way it seems rather obvious, but observers of people's behaviour might find it a salutary guide when deducing causes of behaviour: unwarranted assumptions often result in false conclusions which may be based on otherwise accurate data.

(*c*) *Male medical student*

This subject inhaled scented air while carrying out the tests used in the main experiments. The introspections were written a few minutes after the inhalation was over.

There was a slight Physical Sensation of dizziness at isolated periods during the test, particularly at the start. There was slight impairment of vision in the right eye, for a time, and this can be deemed to have had no effect upon the result of the tests.

Mental alertness was, if anything, slowed, and I got the impression that I was reacting to the stimuli after an appreciable lag—especially the word 'go' which often seemed to come as a slight surprise. Disorientation was not a significant influence in the tests since it only accounted for 1 sec. or so, within other errors.

The tests seemed more variable this time, some being harder than others. It was as if any natural abilities were emphasized or failings brought to light, e.g. mathematical ability, manual dexterity.

On the whole, they seemed to be of the same standard as on the 6 Dec.

(4) *Relation between individual differences in symptom scores and emotional stability*

The emotional stability of 31 experimental and 37 control subjects was expressed in terms of ratings on a five-point scale made by one or other or two teachers who were experienced raters and well acquainted with these subjects. The experimenter's own ratings, made solely on the basis of the subjects' behaviour in the two experimental sessions, correlated significantly with the teachers' ratings, r being 0·443 for the experimental and 0·641 for the control group. The P values on a 1-tail test, used since a positive correlation was predicted, were less than 0·01 and 0·001 respectively. Only the teachers' ratings were used in subsequent calculations, however, as they were regarded as more trustworthy.

Product-moment correlations were calculated between the symptom scores described on p. 185, and the ratings of emotional stability for both groups of subjects, and were found to be not significantly different from zero.

(5) *Relation between individual differences in symptom scores and cognitive impairment*

Quantitative results were available of all experimental subjects' performance in the cognitive tests both before and during inhalation. From these standard difference scores were computed which were measures of the effect—usually deleterious—which the drug had on each subject's performance relative to the rest of the experimental group. The procedure has been described in detail elsewhere (Steinberg, 1953). A product-moment correlation was calculated between these standard difference scores and the symptom scores, and was again not significantly different from zero. Similar calculations were carried out and a similar result obtained from the control subjects. Further, no relation could be detected, in either group of subjects, between the standard difference scores and the ratings of emotional stability.

IV. Summary and conclusions

The findings described may be summarized as follows:

(*a*) Subjects given nitrous oxide reported various psychological and physiological changes. The incidence of such symptoms was significantly greater than among control subjects.

(*b*) Subjects reported many more symptoms in response to the experimenter's specific questions than they had done in the course of free introspective reports.

(*c*) No single symptom was reported universally by all subjects; various degrees of 'dissociation', unsteadiness of movement, tingling, numbness, dizziness, and auditory disturbances were, however, particularly common.

(*d*) Four categories of symptoms strikingly resembled behaviour abnormalities characteristic of psychiatric disorders: illusions and hallucinations, irrelevant or exaggerated thoughts and emotions, perseverations, and 'dissociated' states. Phenomena of this kind were observed particularly in experiments where relatively high concentrations of nitrous oxide were used.

(*e*) No correlation was found between individual differences in the number of symptoms per subject and ratings for emotional stability or impairment of performance in cognitive tests.

While illustrating the wide differences of behaviour abnormalities possible both within and between subjects, the results described clearly do not justify more than the most tentative interpretations.

For example, as regards individual differences in *degree* of disturbance of behaviour one might on various grounds have expected emotionally less stable subjects to be more disturbed by the drug, both in terms of the number of abnormal behaviour phenomena shown and in terms of deterioration in cognitive efficiency. Thus, in connexion with experimentally induced anoxia, several investigators (McFarland & Barach, 1937; Hertzman, Orlansky & Seitz, 1944) have reported that the behaviour of neurotic patients is particularly liable to be adversely affected. Further, it has repeatedly been shown that performance in tests of cogitive efficiency can discriminate significantly between normal and neurotic subjects (Eysenck, 1947, 1952). The lack of correlation in the present research may be attributable to the crudity of the measures used or to the absence of extremes of emotional instability among the student volunteers. On the other hand, susceptibility to a drug may not be a unitary variable, and different aspects of behaviour may be differentially susceptible in different individuals. This would be consistent with Landis & Zubin's (1951) failure to detect reliable relations between subjective feeling states and cognitive test performance under the influence of two different drugs, and with the absence of correlation between the deterioration of three different kinds of cognitive performance during anoxia reported by Birren, Fisher, Vollmer & King (1946). In the present research the intercorrelations between the measures of relative deterioration in the different kinds of cognitive performance tested were also very small.

As for individual differences in the quality and content of behaviour abnormalities induced by drugs, the attractive though relatively unproved hypothesis has often been put forward that they are determined by the individual's existing and latent behaviour tendencies. For instance, Hoch (1951) has expressed this as: 'The best one can say by way of explanation of the action of these drugs is that they somehow injure perception in the

direction of the basic disease process or personality set-up already present.' The data of the present investigation do little more than add to the anecdotal evidence for such a viewpoint; most explicit, perhaps, was the comment by one of the subjects that 'as a result of nitrous oxide you become what you are, only more so'.

The results described must needs leave also unsolved the problem of just how far drug-induced and clinical varieties of abnormal behaviour can ever be said to resemble each other, that is, how far they conform to similar laws in their development, organization and extinction. But even if further investigation were to show that the resemblance is merely superficial, the use of drugs may still have methodological convenience. As Russell (1950) has pointed out, one of the most urgent requirements for progress in the field of experimental psychopathology is the development of appropriate measuring instruments for the various behaviour manifestations themselves. By enabling overtly similar manifestations to be produced artificially in the laboratory, drugs are an obvious aid to such research.

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REFERENCES

BIRREN, J. E., FISHER, M. B., VOLLMER, E. & KING, B. G. (1946). Effects of anoxia on performance at several simulated altitudes. *J. Exp. Psychol.* **36**, 35–49.

BROMBERG, W. (1934). Marihuana intoxication. *Amer. J. Psychiat.* **14**, 301–30.

BUTLER, T. C. (1950). Theories of general anesthesia. *Pharmacol. Rev.* **2**, 121–60.

DESHON, H. J., RINKEL, M. & SOLOMON, H. (1952). Mental changes experimentally produced by L.S.D. (*d*-lysergic acid diethylamide tartrate). *Psychiat. Quart.* **26**, 33–53.

ELKES, J., ELKES, C. & BRADLEY, P. P. (1954). The effect of some drugs on the electrical activity of the brain, and on behaviour. *J. Ment. Sci.* **100**, 125–8.

EYSENCK, H. J. (1947). *Dimensions of Personality.* London: Kegan Paul, Trench, Trubner and Co.

EYSENCK, H. J. (1952). *The Scientific Study of Personality.* London: Routledge and Kegan Paul.

HERTZMAN, MAX, ORLANSKY, JESSE & SEITZ, CLIFFORD, P. (1944). Personality organization and anoxia tolerance. *Psychosomatic Med.* **6**, 317–31.

HOCH, P. H. (1951). Experimentally produced psychoses. *Amer. J. Psychiat.* **107**, 607–11.

HOCH, P. H., CATTELL, J. P. & PENNES, H. H. (1952*a*). Effects of mescaline and lysergic acid. *Amer. J. Psychiat.* **108**, 579–84.

HOCH, P. H., CATTELL, J. P. & PENNES, H. H. (1952*b*). Effects of drugs. *Amer. J. Psychiat.* **108**, 585–9.

HOFFER, A., OSMOND, H. & SMYTHIES, J. (1954). Schizophrenia: a new approach. II. Result of a year's research. *J. Ment. Sci.* **100**, 29–45.

JAMES, W. (1905). *The Varieties of Religious Experience.* New York: Longmans Green and Co.

KLÜVER, H. (1942). Mechanisms of hallucinations. In *Studies in Personality Contributed in Honour of Lewis M. Terman.* New York: McGraw Hill.

KRAEPELIN, E. (1892). *Über die Beeinflussung einfacher psychischer Vorgänge durch einige Arzneimittel.* Jena: Gustav Fischer.

LANDIS, C. & ZUBIN, J. (1951). The effect of thonzylamine hydrochloride and phenobarbital sodium on certain psychological functions. *J. Psychol.* **31**, 181–200.

LEVIN, M. (1946). Transitory schizophrenias produced by bromide intoxication. *Amer. J. Psychiat.* **103**, 229–37.

LINDEMANN, E. (1935). Psychopathological effect of drugs affecting the vegetative system; adrenalin. *Amer. J. Psychiat.* **91**, 983–1008.

LINDEMANN, M. E. & MALAMUD, W. (1934). Experimental analysis of the psychopathological effects of intoxicating drugs. *Amer. J. Psychiat.* **13**, 851–81.

McFarland, R. A. & Barach, A. L. (1937). The response of psychoneurotics to variations in oxygen tension. *Amer. J. Psychiat.* **93**, 1315–41.

McKellar, P. (1954). Personal communication.

McKinney, F. (1932). Nitrous oxide anaesthesia as an experimental technique in psychology. *J. gen. Psychol.* **6**, 195–9.

Marshall, C. R. (1937). The influence of moderate and severe intoxication on remembering. *Brit. J. Psychol.* **28**, 18–27.

Marshall, C. R. (1938). The threshold of unconsciousness. *Brit. J. Psychol.* **28**, 424–9.

Mayer-Gross, W. (1951). Experimental psychoses and other mental abnormalities produced by drugs. *Brit. Med. J.* **2**, 317–21.

Newman, H. W. (1935). Alcohol injected intravenously. *Amer. J. Psychiat.* **14**, 1343–52.

Osmond, H. & Smythies, J. (1952). Schizophrenia: a new approach. *J. Ment. Sci.* **98**, 309–15.

Russell, Roger W. (1950). The comparative study of 'conflict' and 'experimental neurosis'. *Brit. J. Psychol.* **41**, 95–108.

Sloane, B. & Lovett Doust, J. W. (1954). Psychophysiological investigation in experimental psychosis: results of the exhibition of D-lysergic acid diethylamide to psychiatric patients. *J. Ment. Sci.* **100**, 129–44.

Steinberg, Hannah (1953). Some effects of depressant drugs on behaviour. Ph.D. Thesis.

Steinberg, Hannah (1954). Selective effects of an anaesthetic drug on cognitive behaviour. *Quart. J. Exp. Psychol.* **6**, 170–80.

Steinberg, Hannah (1955). Changes in time perception induced by an anaesthetic drug. *Brit. J. Psychol.* **46**, 273–9.

Stockings, G. T. A. (1940). A clinical study of the mescaline psychosis with special reference to the mechanism of the genesis of schizophrenic and other psychotic states. *J. Ment. Sci.* **86**, 29–47.

Wilson, Andrew, Crockett, G. S., Exton-Smith, A. N. & Steinberg, Hannah (1950). Clinical evaluation of effects of drugs on medical students as a teaching method. *Brit. Med. J.* **2**, 484–8.

Zador, Y. (1928). Der Lachgas (N_2O) Rausch in seiner Bedeutung für Psychiatrie und Neurologie. *Arch. Psychiat.* **84**, 1–72.

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THE ROLE OF PAST EXPERIENCE IN APPARENT MOVEMENT: A REVALUATION

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An attempt is made to provide a theoretical framework within which data concerning the effect of stimulus connotations on perceived movement might be evaluated. Jones's & Bruner's (1954) results, and those of our own study, have been examined in this context.

In both cases, it was possible to demonstrate that loading through meaning, to be effective, had to be compatible with generalized past experiences with movement, or (from a structuralist viewpoint) with physiological mechanisms underlying movement perception.

Jones's & Bruner's failure to obtain satisfactory positive results could thus be regarded as due to their underestimation of determinants underlying stroboscopic movement as such. Similarly, successful loading with an adaptation of von Schiller's paradigm could be anticipated, given the fact that equipotential movement alternatives were provided. The extent of the phenomenal results of loading through connotation was assumed to provide a measure of the influence of relevant past experiences.

I. Introduction

Jones's & Bruner's recent paper (1954) reopens an old controversy, by setting out to 'demonstrate some of the ways in which movement-related expectancies condition the perception of movement in a typical stroboscopic experiment' (p. 157).

The Jones & Bruner paper unfortunately does not contain a review of other efforts to deal with the problem, or of the controversy surrounding it. It is one of the objects of the present paper to fill this gap. This involves not only a summary of experimental work paralleling that of Jones and Bruner, but also an attempt at a systematic statement of the theoretical problem of the role of past experience in apparent movement. In this connexion, an attempt will be made to trace out the specific points of difference and agreement between structurally oriented theorists and contemporary empiricists.

Finally, experimental implications will be concretized by a detailed evaluation of the Jones & Bruner data and of an experiment by the authors. The purpose of this section will be to clarify the problem of hypothesis formulation and testing in the area of meaning and movement.

(i) *Experimental background: the use of 'meaningful' stimulus objects*

A direct demonstration of experiential determination of movement perception would be a genetic study showing that such perception was acquired by the individual in the course of his development, as a result of dealings with the environment. This type of research is precluded by the fact that infants already perceive movement at the earliest ages at which experimentation with them is possible. In fact, it has been demonstrated that apparent movement thresholds are considerably lower for children than they are for adults (Gantenbein, 1952; Meili & Tobler, 1931) suggesting that movement perception is genetically a primary category.

Given the practical impossibility of a direct approach, one is faced with the necessity of experimentally attacking the problem of the role of past experience by way of the movement percept of the adult. This indirect approach has taken two lines: (1) the

endeavour to obtain, from protocols, evidence regarding object characteristics in perceived movement; and (2) the demonstration that variations of meaning connotations in the stimulus can influence thresholds or qualities of the movement percept. Since the Jones & Bruner study falls into the second category, the following review will concern itself with experimental literature under this heading.

With regard to object characteristics in movement, it may suffice to note that reports of movement without secondary qualities have been obtained. It has been argued, however, that this 'represents nothing but a bit of interesting observation under fairly specialized conditions' (Neff, 1936, p. 14), and not a fundamental observation. Moreover, the possibility of meaning operating *unconsciously*, as seen by modern empiricists (cf. below), makes this question largely academic.

The first investigator to report the use of meaningful material in apparent movement studies is probably DeSilva (1926), who investigated the role of 'subjective' determinants in movement. Among his findings he specifies that 'in general, an increase in the complexity of the object favors good (stroboscopic) movement. The perception of movement of a solid object is clearer than the perception of movement of a unidimensional object'. Moreover, 'a meaningful, animate pattern gives a clearer perception of movement than an unmeaningful inanimate pattern' (p. 500). DeSilva's findings are not unambiguous, since no indication is provided as to whether more differentiated structure or greater meaning connotation underlies the decreased threshold he reports.

Another investigator using meaningful figures in apparent movement experiments was Blug (1932). The stimulus figures were exposed simultaneously (with a tachistoscope) and movement was obtained by means of rapid brightness changes, as originally suggested by Engel (1928). In meaningful material, Blug reported not only the usual centrifugal and centripetal movements in the field, but 'also apparent movement of the exposed objects. A horse appears to move in the direction suggested in the picture; a bird seems to beat its wings, etc.... The apparent movement is especially pronounced, when its direction coincides with that of the movement elicited by the brightness change' (p. 304). Meaning connotations are reported not only to improve the movement perceived with brightness change, but to enable the perception of movement directions at variance with those observed in response to the brightness alteration *per se*.

A similar type of observation, under different experimental conditions, was made by Frieda Fuchs (1928), who studied projected afterimages of movement. She found that 'with the projection of the afterimage of motion unto...solid objects, there developed a great degree of dependence (of the perceived movement) on past experiences with these objects. The objects appeared to execute, under the influence of the afterimage, movements observed in them in the past. Thus a ball appeared to rotate on its visible axis, *irrespective of the direction of the objective movement from which the afterimage originated*' (pp. 314–15) (*Italics ours*).

Krolik's (1935) paper comprises an exhaustive series of experiments utilizing meaningful figures. The procedure used was that of induced movement, a phenomenon systematically investigated by Duncker (1929) and Oppenheimer (1935). Induced movement has been defined as resulting from the alteration of the distance between two stimuli in an otherwise homogeneous field. It is 'induced' because the movement is allocated irrespective of which of the stimuli is displaced.

Duncker had found that given two figures, one of which does not enclose the other and neither of which is preferentially fixated, movement tends to be equally distributed to the two figures. These would either be perceived as moving away from each other equally, or as alternating on a 50–50 basis. Krolik substituted for Duncker's geometrical figures, slides containing drawings of objects customarily experienced as mobile (car, balloon, ship, train, hand cart, elevator) or stationary in past experience (house, barn, log, beacon). 'With an alteration of the distance between a "house" and a "car",' inquired Krolik, 'will—irrespective of which of these objects were objectively moved under induced movement conditions—the "car" be seen as preferentially moving?' (p. 89). 'If...it should develop that the movement favored by past experience is preferred, we shall be inclined to regard this as a consequence of experience with relevant movement' (p. 53).

Krolik found, under almost all conditions, allocation of movement unilaterally, in favour of the object experienced in the past as moving in the relevant direction. Not only did the 'car' drive away from the stationary 'house', and the 'ship' pass the 'lighthouse', but the 'train' moved horizontally away from the ascending 'balloon', and the 'push cart' behaved similarly with respect to the 'elevator'. Great care was taken to control all possible structural determinants in these conditions, with no significant alteration of obtained results. Even with knowledge, and in a semi-darkened room, it was found almost impossible to offset the effect (p. 83).

Although admitting the positive character of these results, Krolik explains them on the basis of the *principle of enclosure* operating through past experience. His argument will be considered in some detail below.

It remains to touch on an impressive demonstration recently originated by Adelbert Ames, Jr., which has been dealt with in the literature as the 'rotating trapezoid window' (Ames, 1951; Ittelson, 1952; Kilpatrick & Ittelson, 1951). In this demonstration, a flat trapezoid painted to resemble a rectangular window in perspective, appears to oscillate when rotating. The phenomenon can be elicited with a non-painted trapezoid but the impressiveness of the oscillation is greatly diminished, and its radius decreased. The illusion is generally interpreted as the result of cumulative past experiences with rectangles in general, and windows in particular. The weighted average of such experiences is said to make for an unconscious choice (among the possible interpretations of the retinal outline corresponding to the demonstration) of the 'window' interpretation, which requires oscillatory movement. The meaning 'window' thus determines the nature of the perceived movement.

All of the above studies have reported successful modification of movement experiences through the substitution of meaningful material for geometrical figures. DeSilva improved the quality of stroboscopic movement percepts; Blug obtained specific meaningful movements in lieu of brightness-induced gross movement experiences; Fuchs also obtained specific movements through the combination of solid objects with simple movement afterimages; Krolik determined the adjudication of induced movement; Ames reversed the perceived movement direction of a rotating surface. It remains to consider what inferences may legitimately be drawn from these positive findings.

(ii) *Theoretical background: the empiricist position*

Controversy concerning the processes underlying movement perception has largely centred around *phi*, the movement perceived with stroboscopic succession. This phenomenon has been regarded by investigators of the gestalt school as a fundamental source

of information concerning perceptual processes. Explanations of *phi* in this framework have been couched in physiological terms. The principal hypotheses are probably Wertheimer's (1925) 'short circuit' theory, Koehler's (1923) 'Stromfaedensaeule' displacement hypothesis, and Koffka's (1935, p. 280 ff.) discussion of process fusion. Criticisms of this emphasis as a whole have largely centred around the fact that apparent movement is subject to influence through attitudinal factors, and that hence it could not be an elementary physiological product. (Cf. DeSilva, 1926; Linke, 1929; Higginson, 1926; Neuhaus, 1930; Peterman, 1932; Pieron 1952.)

Gestalt psychologists have not only acknowledged the effectiveness of attitude in experimentation under the heading of 'attitudinal set' (Cf. Wertheimer, 1925, pp. 44–52) but have even specified it as a determinant in marginal situations (e.g. von Schiller, 1933). This same position has been taken in relation to past experience. Thus Wertheimer makes the parenthetical statement that 'it is certainly correct that past experience can influence conditions of stroboscopic perception' (p. 79). Metzger more recently indicates that 'experience has been assured a role as an independant force'. However, he adds: 'It can only manifest itself in conjunction with other forces, at times very powerful forces, which have their origin partly in Gestalt-relationships and partly in the condition and attitude of the observer'. Furthermore, 'without the existence and operation of (gestalt) laws experience could never be accumulated nor applied' (1936, p. 154).

In view of this position of gestalt theories, and since no empiricist would deny the physiological substratum of perception, the fundamental question has been reduced to one of relative primacy or importance of learning processes and physiological mechanisms, instead of one of exclusive determination. The difficulty of assigning priority is pointed up in Krolik's discussion of his experimental results.

As will be recalled, Krolik found that movement (in induced movement situations) was allocated in line with meaning connotations of the two subjects. He concluded, however, that these apparent effects of past experience are actually the result of the operation of the 'principle of enclosure'. This is Duncker's structural law for movement allocation which simply states that 'the enclosed object moves more than the enclosing one' (1929, p. 205). Krolik bases his argument (1) on a single empirical finding with a special presentation (the 'enclosing landscape' slide), and (2) on an *ad hoc* theoretical construct, that of the 'apparent' (non-manifest) frame of reference.

In the 'enclosing landscape' condition the two stimulus objects extended over a screen directly in front of the subject. One of these objects was a small 'house' in the centre of the field. The other, consisting of 'movable' components such as clouds, a zeppelin, water, boats and ships (together with a barely perceptible shore line) completely enclosed the 'house'. When the displacement took place it was the house which appeared to move, despite its stationary connotations. From this fact Krolik (pp. 90 ff.) concludes that experience as a determinant is secondary to the structural law of enclosure.

This conclusion, however, does not necessarily follow from the observation. Not only does past experience not ordinarily provide objects travelling at identical speeds (and not relative to each other) but no empiricist would deny the possibility of eliciting movement in stationary stimuli. More important, it can be convincingly argued that in past visual fields objects (figures) have been invariably experienced as moving against stationary backgrounds (ground). The prediction of Krolik's observation could thus be made from empiricistic considerations.

Equally susceptible to rebound is Krolik's argument about 'apparent' frames of reference. Krolik observes that meaningful figures, in contradistinction to geometrical ones, seem supported on invisible surfaces, and appear to be surrounded by non-visible 'atmosphere'. He concludes that each object has its own frame, with reference to which it moves. These frames are not provided in the presentation, but are psychological (unconscious) addenda. Movements according to experience thus occur to 'facilitate the reproductive formation of apparent...enclosure' (p. 97). In other words, 'the effect of past experience in our experiment can be easily categorized under the autochthonous laws, as these were formulated by Duncker in his law of "allocation of movement" for structural movement constellations: "phenomenal movement is the displacement in a natural frame of reference"' (p. 100).

The observation concerning invisible support and apparent atmosphere can, however, simply be regarded as meaning that, for past experience to operate (in the shape of object connotations) stimuli have to be susceptible to interpretation (at some level) as concrete objects. Such interpretations would involve unconscious size-distance assumptions.

The Ames Demonstrations, one of which has been discussed above, are in part designed to make the point that the "thatness" of an object is an experiential product, and its 'thereness' (hence, its movement) is an inseparable corollary of it. From this point of view it can be successfully argued that without the assignment of the meanings 'car', 'house', etc., the different apparent distances of these objects could never result. Since Krolik does not provide for the origin of his 'frames', his conclusion is not an alternative to such an argument.

The above would certainly be more in accord with the bulk of Krolik's results, and his original hypotheses, than the *ad hoc* explanation he presents.

We have here referred ourselves to generalized past experiences, such as that objects move relative to backgrounds and have implicit sizes and distances. Experiential effects of this kind were not allowed for by nineteenth-century empiricists of the associationist school. Contemporary empiricism, however, is not couched in terms of isolated object connotations, but rather of weighted averages of experiential sediments of all kinds, acting inseparably in the interpretation of intrinsically meaningless receptor events (Cantril, 1950; Ittelson & Cantril, 1954; Kilpatrick, 1952*a*). (These sources, representing the 'transactional' approach to perceptual theory, are cited as illustrative of the empiricist position, not as its exclusive exponents. Other writers have taken compatible positions with regard to perception in general and apparent movement in particular. Among the latter are Gemelli and his students, as well as the Innsbruck group—Erismann and Lehmann. Space limitations preclude a discussion of the specific points of view of these investigators.) Past experience in this context would not only operate on a 'house-is-stationary' and 'car-is-mobile' level, but would make for the interpretation of 'house' and 'car' in the first place, and of the relevant spatial and ordinal characteristics.

Stroboscopic presentations would be expected to 'move' because of cumulative experiences with all kinds of sequences. That is to say, the infant's experiences from his earliest days with rapid succession of sensory data in successive receptor locations have responded to the interpretation 'motion'. This—it would be postulated—has created a very strongly weighted unconscious translation of ordinal events into 'movement'. Given the basic character of this unconscious translation, one would expect it to outweigh object connotations, when the two orders of experience are in conflict (as they are not in

every day experience).* The prediction, in this situation, would be the reverse of what an associationist might have expected, and in line with 'gestalt' predictions in the same case. The only point of difference between the contemporary empiricist and the structuralist would concern the 'ordinal' determinant, which the latter would regard as a structural law of physiological origin, and the former, as an experience-derived 'assnmption'. (The same would hold true of the so-called 'autochthonous' factors in space perception.) The possibility remains that this issue, given its generality, may eventually disintegrate into a verbal distinction.

Since 'gestalt' theorists admit the feasibility of determining movement percepts by object connotations, when so-called 'structural' factors are neutral, and empiricists do not make further claims for object connotations, experimentation in this area would not be critical for the empiricist-nativist controversy. The only type of critical experiment would be a direct (genetic) approach, which is not practicable (cf. p. 195, above).

The possibility that experimentation with meaningful material in movement may not be decisive for the isolation of the role of experience does not invalidate such work. For structurally oriented investigators, this type of experimentation may be regarded as gauging the extent of the potential effectiveness of experience (as a perceptual determinant) when set against other (physiological) determinants. For the empiricist, the relative weights assigned to different experiences in perceptual organization would be under investigation.

The following will attempt to translate the empiricist formulation into experimental terms through an evaluation of data. The emphasis will be on the qualifications a realistic working hypothesis in this area should meet, in accordance with the above.

(iii) *Limitations to the experimental demonstration of the role of experiential connotations*

It has been implied above that in line with both nativistic and contemporary empiricist theory, the stroboscopic sequence *a*1–*b*2 would constitute a mandatory condition (if presented above *phi* threshold) for the perception of movement from *a* to *b*, and no other. The introduction of content suggesting a reverse movement would not be expected to offset this direction, although it could be expected to raise the movement threshold.

This type of presentation would involve, for the empiricist, a conflict between heavily weighted assumptions of long-term origin (as to movement in general) and relatively weak assumptions activated by meaning connotation.

In order for movement to occur in line with specific presentational content, the latter would have to suggest a sequence compatible with what has been learned regarding movement in general. The conflict, in order to be resolved in favour of meaningful connotations, has to be between two or more empirically plausible potential movements. The role of content in experiments featuring stroboscopic movement, in other words, has to be that of loading one of the potential movement alternatives (as more in line with past experience

* This occurs, for example, in the 'rotating trapezoid window' with a cross-bar attached. The bar appears to 'cut through' or 'bend around' at apparent turning points despite object connotations to the contrary, in order to allow for meaningful sequence. Interestingly enough, this non-object behaviour is subject to experimental control through variations of object connotations. Interpretations of 'rubber' or 'steel' can determine 'bending', or 'cutting' (Kilpatrick, 1952*b*).

than another), the prediction being that such loading would make for preferential movement in the suggested direction. Content would play the part of reinforcing one of the possible resolutions of a choice situation.]

(iv) *The Jones and Bruner study*

In the Jones & Bruner experiment, where 'the relationship of stimulus context to postulated general expectancies' was under investigation ('stimulus context' being defined as the 'experimentally manipulated stimulus framework in which movement occurs' and 'expectancies' as 'the pre-established network of hypotheses about movement in the subject') the *primary* context would be the extent and direction of movement with neutral (equally weighted) figures, and the *basic* 'expectancy' that of *a* to *b* movement of an *a*1, *b*2 presentation. Degree of departure from this basic movement would represent success of loading through meaningful connotation.

It may be relevant to examine the experimental data, as reported, for degree of success in loading.

The first of the Jones & Bruner experiments contrasts the apparent movements of a meaningful figure (stick man) and a meaningless one (nonsense object) relative to each other. The stroboscopic presentation is a three-stage one, with each object ending at the point of departure of the other. Differential movement favouring the 'stick man' is predicted. Such differential movement is, also, observed—at first—but 'on the fifth trial. . . no longer obtains. Eight of the subjects saw the (two) moving at an equal speed, and eight of the remaining ten saw the nonsense object as moving faster' (Jones & Bruner, 1954, p. 159).

Jones & Bruner dismiss the reversal with a reference to 'satiation', and conclude that 'a man-in-motion has a higher subjective probability value than a thing-in-motion' (p. 159). The following 'hypothesis' is regarded as 'confirmed': 'When two objects cover an identical distance objectively, the object which has more frequently been seen in motion will be seen initially as moving faster and farther' (p. 165).

It is a matter of possible dispute whether a restatement in general terms of a finding under specific conditions is methodologically justified. However, if it were, one might equally well conclude that figures which have not been observed to move in terms of experience will in general tend to move faster and farther after a few initial trials, requisite for structuring of the percept.

In point of actual fact, preferential movement of a figure denoting a 'moving' object as against one that does not carry such denotations has been obtained (with relative displacement) under induced movement conditions. The effect there reported is unambiguous, and the 'non-moving' figure is seen as stationary throughout (Krolik, 1935, cf. above p. 197).

The Jones & Bruner presentation: (1) Presupposes that stroboscopic intervals would not *per force* elicit the same degree of movement in both figures, for which no rationale is offered. The movements called for would not *a priori* have to be differential since they are not mutually exclusive (cf. Higginson, 1926, who has obtained different movements over the same space). *Since there is no forced choice or conflict, and the presentation is one which permits the perception of a configuration involving two discrete movements, the hypothesis is far from convincing.* (2) A greater difficulty in identification of the nonsense figure may underlie the initial reports. (3) Any remaining possible effects in favour of the 'stick man'

are neutralized by having this figure larger, which would tend to make for slower apparent movement (Brown, 1931). Hence, probably, the reversal.

The utility of a 'nonsense figure', of course, rests on the presupposition of its meaninglessness. Whether no meanings are actually assigned to such figures is open to question. In fact, Jones & Bruner's 'nonsense figure' looks almost human, and its presentation in conjunction with the 'stick man' may reinforce this significance.

Jones & Bruner's second experiment compares the subjective goodness of movement in meaningful and meaningless presentations, after one stage (a middle terminal) is omitted in both. It is reported that the break is felt more in the control condition. However, since fully half the controls did not report any change, and 21% of the experimental group did, a statistical test of the difference appears desirable. This is not provided, because 'inadvertently the original data for this experiment were destroyed' (Jones & Bruner, 1954, p. 160). Therefore, although the paradigm is an interesting one, the conclusions that 'the more probable and practised the movement, the more adequately will the movement experience maintain itself under sub-optimal conditions' (p. 165) may require further substantiation. Again, the basic nature of apparent movement perception as such may make the obtaining of a significant difference here a difficult task.

The third of the four experiments described in the Jones & Bruner paper featured a diagonal line, and two sets of three circles successively exposed on it in a downward direction. The circles contained an inset arc, in three different positions (left, upper right and lower right) in one case, and one constant location (left) in the other. The differential quality of the movement reported in these two presentations (predominantly 'rolling' as against a modal 'skidding') is interpreted as a function of expectancy, with the circle being supposedly interpreted by the observer as a 'ball'. Unfortunately, standard structural explanations for movement direction can be brought to bear on the difference in movement quality, such differences being generously represented in the literature. The 'ball' interpretation is hypothetical, and the experiment therefore is far from critical.

The fourth and last of the reported conditions sets itself precisely the task outlined above as the realistic test of experience effects. 'The general hypothesis is that where there is an option in the path of apparent movement, that path of movement will occur, other things being equal, that most conforms to an established expectancy' (Jones & Bruner, p. 163). The basis for the presentation used in this experiment is supplied by Ternus, who found that dots successively exposed as (1) spatially separated, (2) together, and (3) separated, could be optionally seen as 'crossing over' or 'bouncing' off each other. These dots, in the Jones & Bruner condition, were shown in the context of (*a*) 'baseball players' (as baseballs), and (*b*) 'billiard players' (as billiard balls). In the first case, they were expected to 'cross over', and in the second to 'bounce off' each other.

Although the results 'tend to support the prediction' they failed to do this conclusively, i.e. the frequency differences fell short of significance. This failure to obtain fully satisfactory results may be at least partly due to the lack of realism in the presentation, testified to by the fact that Jones & Bruner report that 'if the situation was *not understood at all* (*as was frequently the case with the pool players*) the initial stimulus card was taken from the episcope and shown to the subject' (Jones & Bruner, p. 163). The procedure referred to, incidentally, is open to objection, since it may be regarded as tantamount to suggestion. To assert that experiential factors operate, necessitates evidencing their

spontaneous intervention in perceptual processing. No basis would otherwise be provided for drawing a line between effects of past experience and experimentally induced set.

The above considerations serve to place limitations on the Jones & Bruner data as adequate demonstrations of the role of meaning or the effect of past experience. They also may illustrate some of the difficulties in this type of experimentation, and the delicacy of the experimenter's task.

II. CHOICE AS A PARADIGM: AN EXPERIMENTAL ILLUSTRATION

The experiment reported below is derived from a rationale similar to that of the fourth Jones & Bruner experiment, and analogous to Krolik's (1935) induced movement study. The object was that of investigating the effect of meaning in a situation where adjudication of movement is not determined, i.e. where a choice is open.

The experimental framework, identical to that used by von Schiller (1933) in his study of assimilation in apparent movement, is analogous to Linke's 'double cross' presentation. This is a stroboscopic presentation consisting of a vertical and diagonal cross, flashing alternately in the same position. A single cross is seen to rotate, under these conditions, in one of two equipotential directions (Linke, 1929, pp. 326–7).

Von Schiller posed himself the question: 'Under what conditions will one movement percept (direction, kind of movement) be preferred out of two geometrically equivalent possibilities in the objectively given stimulus constellation? Generally formulated: What determines the resolution of a set of alternative stroboscopic movements?' (p. 181).

As his experimental condition, von Schiller used an arrangement in which the first figure was flashed in the centre of the presentation, and the *b* figures (simultaneously) around it. Variations were introduced by manipulating the similarity of one or more of the *b* figures to the *a* figure, altering *a–b* distances selectively, or changing the total pattern.

Von Schiller found that in general the *a* figure would tend to divide and radiate *to any and all b figures*, a tendency which he called 'tendency to total assimilation'. However, *preferential movements* sometimes occurred, notably over shortest distances and to the most similar figures. Selective attention to parts of the presentation made for preferential movement favouring the member attended to.

The following exploratory experiment, carried out by the authors,* introduced meaningful connotations into von Schiller's paradigm. An arrangement of three figures was flashed in *b a b* sequence, with the middle one preceding the outside figures, which were flashed simultaneously.

Flashes had a duration in the order of 50 msec. The intervals used were 40 and 70 msec. Intensity was systematically varied, with an arbitrary optimum value of 11 foot-candles being used for the bulk of observations.

The stimulus objects were *c.* $1\frac{1}{2}$ in. long, and were flashed $1\frac{1}{2}$ in. apart, 7 ft. from the observer. Observations were carried out binocularly, in a totally darkened room, with the observers dark adapted. A total of eight observers was used.

The apparatus was built around a mechanical timing device operating through a set of relays, and a series of variable transformers. The viewing unit consisted of a box divided

* This research was conducted under Office of Naval Research Contract N 6 onr 27014 with Princeton University. The opinions expressed, however, are those of the authors, and do not represent the opinions or policy of the United States Navy.

into three compartments by tin partitions, each containing two $7\frac{1}{2}$-watt filament bulbs facing a pane of diffusion glass. Between these boxes and the coated glass fronting the entire unit, a slit allowed for the insertion of a photographic slide containing (white on black) figures.

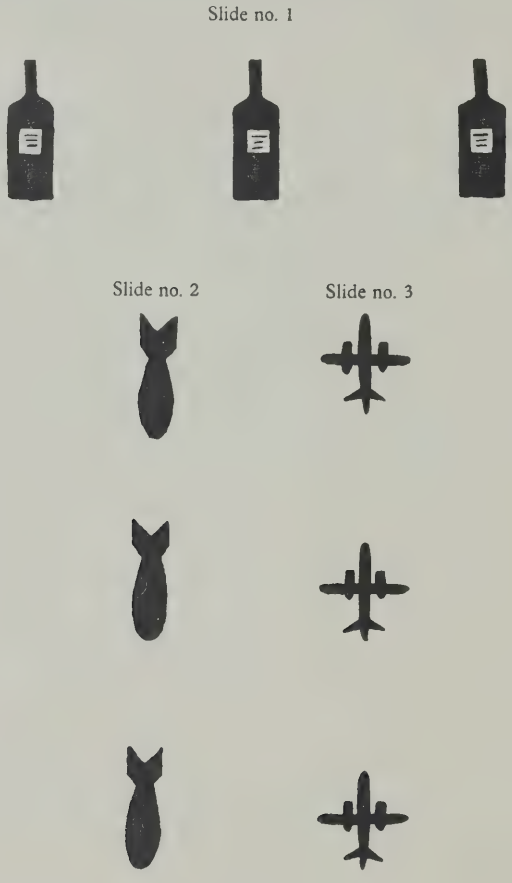

Fig. 1.

Three slides were used for the purposes of the experiment. One of these was a control slide, and consisted of a series of three bottles (Fig. 1). The rationale for this presentation was that meaning-connotations of a bottle are relatively neutral as regards movement direction. It was further considered that neutral meaningful objects carried a methodological advantage over abstract or nonsense figures, in that the possibility of arbitrarily assigned meanings is minimized, and the situation is more strictly analogous to the experimental one.

Of the two experimental slides, which were flashed vertically (in order to be comparable, and serve as additional control to each other), one contained solid outline drawings of three bombs (pointed downward), and the other featured three two-engine airplanes (nose upward). In order to preclude, in so far as possible, the operation of structural factors, the bottom of the bombs was rounded, and the wings of the planes were located toward the centre of their bodies. Although this has not entirely eliminated the possibility of

structural determinants operating, the probability is considerably reduced. Given the fact that all three presentations are symmetrical, that distances are equivalent and the three figures identical, von Schiller's results would permit the prediction of movement outward from the centre unless meaning intervened to the contrary. It was postulated that, in the experimental slides, the direction in which the objects represented would move according to past experience would be that in which preferential movement (of the kind elicited by shortest distance or greater similarity) would be seen by observers.

III. RESULTS

The results, for the eight observers used, have been summarized in Table 1. As will be noted, five of the observers saw symmetrical movement in the control slide. For these observers, the middle 'bottle' was stationary, while the other two appeared to move away from it. Two of these observers saw occasional preferential movement, mostly toward the right, where the experimenter's red light was located.

Table 1. *Experimental results*

Number of subjects reporting each observation (total $N=8$).

Types of observation	Slide 1 ('Bottle' presentation)	Slide 2 ('Bomb' presentation)	Slide 3 ('Airplane' presentation)
Movement:			
(1) Symmetrical (bilateral)	5	1*	0
(2) Unilateral	0	6	5
(*a*) Exclusively	—	4	4
(*b*) Preferentially	—	2	1
Direction: Up	—	0	5
Down	—	6	0
(3) Other movement:	2	0	1*
Total movement	7	7	6
Total succession	1	1	2
Total subjects	8	8	8

* No recognition. Figure seen as geometrical or different.

Of the miscellaneous three (out of eight) observers, one saw the bottles 'wiggle' on the spot, another saw them execute complex three-dimensional loops, while the third reported two bottles, flashing alternately. The only significant difference between the modal response elicited by the control slide, and von Schiller's data is the fact that it is not the *a* bottle which moves to the *b* bottles, but the latter which move away from the centre. (This is the kind of movement described by Wertheimer (1925, pp. 30–4) as 'dual partial movement'.) This may perhaps be interpreted as a function of the connotation 'bottle', which makes it difficult for the figure to split and move in two opposite directions.

Slide no. 2 (the 'bomb' presentation) elicited only one set of 'parting movement' responses. These came from an observer who did not recognize the 'bombs', but saw 'three vertical lines', which appeared to part vertically. The other six observers who saw movement in this presentation, reported preferential movement down. Two of these saw three bombs falling, while the others saw the bottom two bombs moving down, with the top one sometimes stationary, and sometimes 'joining in'. Occasionally only one bomb was reported, which was seen as moving from top to bottom. The two remaining observers who saw movement reported downward movement at higher brightnesses, and parting movement (up and down) at lower intensities.

The picture is closely analogous for the 'airplane' presentation (slide no. 3). Of the four observers who saw movement exclusively upward, two reported all three airplanes as participating, while the other two noted movement almost exclusively by the top plane. One observer saw movement predominantly upward at higher brightnesses, and parting at lower intensity values. The remaining observer who saw movement reported 'three ballerinas, jumping upward on their toes'.

These data clearly show that even with a marked lack of realism in the presentation (which in the case of the airplane is partly due to the effort to achieve structural neutrality) and the sketchy character of the objects, meaningfulness in terms of past experience markedly contributed to the determination of movement direction. Moreover, the effect was more striking than could be predicted, since the tail end figure was at times seen to move in a direction opposite to that in which it would move with neutral objects, such as those used by von Schiller. Thus the perceived movement is more preferential than could be anticipated.

An interesting difference in reported movement direction at times manifested itself with varying brightness, increased intensity showing itself as favouring the determining role of meaning. At higher intensity movement was also reported as faster, which may have a bearing on the fact. (The problem of apparent velocity in stroboscopic presentation of different brightness still awaits systematic investigation.)

The above experiment may be taken as demonstrating the utility of von Schiller's paradigm for investigations of the effect of meaning on direction and quality of perceived movement.

References

Ames, A. Jr. (1951). Visual perception and the rotating trapezoidal window. *Psychol. Monogr.* **65**, Whole no. 324.

Blug, A. (1932). Neue Untersuchungen über Scheinbewegung bei tachistoskopischen Beobachtungen. *Z. Psychol.* **127**, 290–324.

Brown, J. F. (1931). The visual perception of velocities. *Psychol. Forsch.* **14**, 199–232.

Cantril, H. (1950). *The 'Why' of Man's Experience.* New York: MacMillan.

DeSilva, H. R. (1926). An experimental investigation of the determinants of apparent visual movement. *J. Exp. Psychol.* **37**, 469–501.

Duncker, K. (1929). Über induzierte Bewegung. *Psychol. Forsch.* **12**, 180–259.

Engel, P. (1928). Tachistoskopische Expositionen und Scheinbewegungen. *Z. Psychol.* **107**, 273–313.

Fuchs, F. (1928). Experimentelle Studien über das Bewegungsnachbild. *Z. Psychol.* **106**, 267–315.

Gantenbein, Maria-Martha (1952). Recherche sur le developpement de la perception du movement avec l'age. *Arch. Psychol.* **33**, 198–294.

Higginson, G. (1926). Apparent visual movement and the gestalt. *J. Exp. Psychol.* **9**, 328–52.

Ittelson, W. H. (1952). *The Ames Demonstrations in Perception.* Princeton: University Press.

Ittelson, W. H. & Hadley C. (1954). *Perception: a Transactional Approach.* Doubleday Papers in Psychology, no. 7, Doubleday, N.Y.

Jones, E. E. & Bruner, J. S. (1954). Expectancy in apparent visual movement. *Brit. J. Psychol.* **45**, 157–65.

Kilpatrick, F. P. (ed.) (1952*a*). *Human Behavior from the Transactional Point of View.* Institute for Associated Research, Hanover, N. H.

Kilpatrick, F. P. (1952*b*). *Assumptions and Perception: three Experiments*, Institute for Associated Research, Hanover, N. H. pp. 153–73.

Kilpatrick, F. P. & Ittelson, W. H. (1951). Three demonstrations involving the visual perception of movement. *J. Exp. Psychol.* **42**, 394–402.

Koehler, W. (1923). Zur Theorie der Stroboskopischen Bewegung. *Psychol. Forsch.* **3**, 319–96.

Koffka. K. (1935). *Principles of Gestalt Psychology.* N.Y.: Harcourt Brace.

KROLIK, W. (1935). Über Erfahrungswirkungen beim Bewegungssehen. *Psychol. Forsch.* **20**, 47–101.
LINKE, P. F. (1929). *Grundfragen der Wahrnehmungslehre.* Munich: Ernst Reinhardt.
MEILI, R. & TOBLER, E. (1931). Les mouvements stroboscopiques chez les enfants. *Arch. Psychol.* **23**, 131–156.
METZGER, W. (1936). *Gesetze des Sehens.* Frankfurt: Kramer.
NEFF, W. S. (1936). A critical investigation of the visual apprehension of movement. *Amer. J. Psychol.* **48**, 1–42.
NEUHAUS, W. (1930). Experimentelle Untersuchung der Scheinbewegung. *Arch. ges. Psychol.* **75**, 315–458.
OPPENHEIMER, E. (1935). Optische Versuche über Ruhe und Bewegung. *Psychol. Forsch.* **20**, 1–46.
PETERMAN, B. (1932). *The Gestalt Theory and the Problem of Configuration.* London: Kegan Paul.
PIERON, H. (1952). *The Sensations.* London: F. Mueller.
VON SCHILLER, P. (1933). Stroboskopische Alternativversuche. *Psychol. Forsch.* **17**, 180–214.
WERTHEIMER, M. (1925). Experimentelle Studien über das Sehen von Bewegung. *Drei Abhandlungen zur Gestalttheorie*, Verlag d. Philos. Akad., Erlangen, 1–105.

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THE STATISTICAL THEORY OF FIGURAL AFTER-EFFECTS AND ACUITY

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Osgood & Heyer explain the shift of a contour by the prolonged inspection of another in the vicinity by assuming a spread of excitation in the cortex, associated with each contour. This spread of excitation is normally distributed. The contour itself is 'perceived' at the peak of the distribution. The shift in the second contour occurs because of an area of depressed excitability, left by the first, which will tend to skew the second distribution of excitation, and its peak, away from the locus of the first. It is argued that if the two distributions are to subtract from each other successively, they will add up to a single peak if placed on the cortex simultaneously. Thus any interaction will occur only if the two contours are below the threshold of separation and any shift will be a mere fraction of this threshold.

Various amendments are considered which might enable the theory to evade this consequence. The neurophysiological basis (Marshall & Talbot) for Osgood & Heyer's theory is also questioned. A hypothesis to explain figural after-effects and the after-effects of seen movement is briefly suggested.

I. Introduction

When a visual contour has been fixated for a minute or two, other contours placed in the vicinity are phenomenally shifted away from the locus of the first contour. This shift is not largest the nearer to each other the two contours are; the maximal after-effect is secured when the two lines lie at a small distance from each other. This fact is known as the distance paradox. Though workers such as Gibson (1933) obtained such effects, it remained for Köhler & Wallach (1944) to investigate them thoroughly and to clarify their genesis. Köhler & Wallach's empirical work on figural after-effects, as these illusions are called, has largely been confirmed and extended; their theoretical interpretation has been hotly disputed. The neurophysiological assumptions peculiar to the theory have been amply criticized, both experimentally and theoretically, by Lashley, Chow & Semmes (1951). Osgood & Heyer (1951) have made an attempt to construct an hypothesis consistent with what they take to be the facts of neurophysiology (that is, the speculations of Marshall & Talbot, 1942). This schema, it is claimed, can account for the facts observed equally as well as those of Köhler & Wallach. Like many theories in psychology, such as Hull's (1952) and Hebb's (1949), it has implications which are never brought out by the author and which render the theory self-defeating. Absurd consequences are generated and pass unnoticed. However, to overlook these is all too easy, and the author will be very sorry if any criticisms he makes obscure the real merits, the ingenuity and the simplicity of Osgood & Heyer's theory.

II. Criticism of Osgood and Heyer

Any theory of figural after-effects has, as its main task, something which appears almost too simple—to explain why one line, after being exhibited for some time, begins to push all others away. Osgood & Heyer picture this in terms of a mechanism somewhat as follows.

The authors assume 'that the apparent localization of a contour in subjective space coincides with the location of maximal excitation in area 17' (Osgood, 1953, p. 236).

'Drawing directly on the work of Marshall & Talbot (1942), we may assume that the representation of a contour in the projection cortex is a normal distribution of excitation symmetrical about its own axes transversely and extending as a "ridge" throughout the longitudinal extent of the contour'. The shift in excitation, representing another contour placed close to the locus of a previous one, occurs because 'under constant fixation of a figure, the cells in area 17 mediating the on-off activity will become differentially adapted as negatively accelerated functions of (*a*) the rate of their excitation and (*b*) the time through which they are excited' (Osgood, 1953, p. 238). Hence 'the locus of maximal rate of fire in the distribution representing a T-contour in subjective space can be shifted because of the differentially adapted region in which it falls' (*ibid.* p. 238). This would explain a shift. Unfortunately for the theory such a shift could hardly be seen.

The adaptation is held to take place in the previously excited area. Hence, only a contour falling within this area would show any shift. But consider the two contours displayed simultaneously. Their distributions of excitation would summate if their areas of excitation overlapped. Now it is anybody's guess how far the two distributions must be merged before they are treated as one and the same distribution, but there must come a point somewhere before the combined distribution loses its two humps and summates into one very large one. Approximately the latest point, given an equality of the two distributions, at which discrimination can occur, is when the two points, which are half the height of the peak of the two distributions taken separately, are brought into coincidence, given a normal distribution. It will be the distance between the two peaks just before this occurs that would give such a system, at its best, a threshold for the discrimination of two adjacent lines. These considerations carry implications about the figural after-effects to be predicted from this theory.

(1) By inspection of Osgood's figures, the optimum distance for a maximum effect (the distance paradox) is well within the threshold, as calculated above, for the discrimination of two adjacent lines.

(2) Needless to say, the maximum displacement occurring on this theory (again taking Osgood's diagrams), is very much smaller than the optimum distance for a maximum effect. This means that it is much smaller than the threshold for the discrimination of two adjacent lines. It becomes difficult to understand how such displacements could be reported and to be seen to be decreasing by the subjects. It could be said that the shifts were large because thresholds were high on account of the conditions of the experiment. But however high they were and however poor the conditions of the experiment, the displacements could still only be below the thresholds for angular separation there and then. The trouble with the theory is not that it necessarily predicts too small an after-effect but that it must always predict an effect which is presumably too small to be seen.

(The estimate of threshold here is based on that for angular separation. Yet, with the methods of estimating size and distance typically used in the experiments on figural after-effects, the thresholds for angular separation were almost certainly smaller. It might be held that the displacements in the figural after-effect experiments could be smaller than the minimum separable and, yet, be visible. The threshold for the visual perception of the smallest misalinement is quoted by Polyak (1941) to be from 8″ to 2·5″ of arc and even less. But this is a different judgement; in the typical after-effect experiment, Weber's law should give us a far better approximation to what the difference limen was. Nevertheless, however we estimate acuity in this situation, the fact remains that Hammer (1949)

reports a figural after-effect of 2′ where the expected minimum separable is of the order of 1′.)

Is there defence to this for Osgood & Meyer? The force of the criticism rests on a connexion between thresholds for separation and shifts of peak on a 'statistical theory'. One contour can only displace another if their distributions of excitation overlap to a large degree. But if they do overlap to any extent, the distance between them must be subthreshold (or very near threshold). Further, the amount of displacement possible on this theory must always be a fraction of the distance between the contours. Thus, any displacement should be too small to be detected by the subject (or at any rate very much below his threshold for the separation of two contours).

It might be argued that the two distributions of excitation, representing the two contours, do not summate though they overlap and, therefore, we cannot draw any conclusions concerning thresholds from the theory. But clearly, unless each unit of excitation carries some sort of identification mark saying to which contour it belongs, effective summation must occur. If the units of excitation are identical, two peaks of excitation superimposed must add up to one. If there are various types of excitation, this is an important assumption which Osgood & Heyer ought, at least, to have mentioned. It would, however, weaken some of their claims to physiological orthodoxy and, with it, some of their advantage over Köhler & Wallach.

However, any assumptions to yield such properties would complicate the theory immensely. If there are types of excitation which do not interact when simultaneously present, why should they interfere when they occur in succession? Nevertheless, it might be thought that Osgood & Heyer could not have contemplated summation of two overlapping processes to produce the appearance of one process in between and that there must be some easy, but implicit, assumption which would avoid the difficulty. Unfortunately, Osgood uses this very idea of summation (1953, p. 245) to explain stroboscopic movement. 'We already know that smooth, continuous apparent movement can be obtained from discrete, disparate stimulation when they fell within the same region over which lateral contour effects are obtainable. This suggests the hypothesis that apparent movement arises when two contours interact when their excitation peaks are mutually shifted. Since stimulus (*a*) precedes stimulus (*b*) by a brief interval, it follows that the distribution representing (*a*) will be in the process of decay (decreasing rate of discharge) while that representing (*b*) is in the process of development (increasing rate of discharge). The necessary condition for smooth, continuous apparent movement—optimal movement—would be that in which summated distributions produce a single peak that *continuously* shifts from (*a*) to (*b*). . . . The arrow indicates the shifting position of the single perceived contour'.

(Curiously enough, the lateral contour effects which suggest these summations of excitation to Osgood are the ones described by Fry & Bartley. In his own words (also p. 245) 'it has been shown that the presence of one contour may inhibit the formation of another with 4° *distance.*') Nevertheless, here we have a passage which shows clearly that Osgood contemplates a summation of two separate excitatory processes occurring simultaneously to produce the 'perception' of some intermediate contour. It is true that, when this summation is occurring centrally, the peripheral conditions are not the same as there are not two lines impinging on the retina simultaneously. It could be claimed that, when two contours are simultaneously displayed, the distributions are narrowed by some

means (some sort of mutual inhibition) so that they do not overlap. This argument would increase the radius of action of the after-effect in relation to the threshold, and break the dependence on which this criticism is based. But even this addition to Osgood and his postulates can hardly be a way out because of the facts of self-satiation. How could two non-overlapping excitations act on each other in such a way as to produce a shift? Osgood & Heyer would have to make *ad hoc* assumptions of some complexity to adopt this line of defence.

Superficially, the most attractive way of breaking the dependence of threshold on amount of after-effect is by altering the shape of the distributions of excitations which Osgood & Heyer posit. Oddly enough, this ancient panacea which has, in its time, revived a great many theories just as they were threatened with demise does not seem to help a great deal here. We should have to invent a curve giving a separation threshold of 1′ 15″ and a figural after-effect of 2′ when the two peaks were 22′ apart, of such a shape as to yield a distance paradox. This would be a good problem for producing a nervous breakdown in a calculating machine!

These two figures for after-effect are taken from Hammer, whose work Osgood also quotes (1953, p. 241). The figure for acuity is corrected for angular distance of 1° 30′ from the ideal foveal centre from Wertheim's curve, as reproduced by Polyak, to make a generous allowance for the fact that Hammer was working roughly this amount out to the periphery (Polyak, 1941, fig. 99).

A related way of escape, which would alter the theory still further, is that the two curves which are interacting are unlike both in shape and extent. Two possibilities, which are not mutually exclusive, are that, during eye movements, a peak of excitation travels hither and thither over the surface of the cortex leaving a band of inhibition very much broader than its own extent at any moment, and (or) that there is an area of inhibition much larger than, and surrounding, the strip of excitation and which persists longer. The second possibility has, indeed, much evidence to recommend it. However, it seems that both suggestions founder on the same objection. The curve of inhibition, though broader than that of the superimposed excitation, cannot be higher. (Otherwise, the second curve would be completely obliterated in some positions.) This means to say that the incline of its shoulders cannot be very steep. On the other hand, even a vertical incline of inhibition could not shift the peak of an excitatory distribution more than half the breadth of the distribution. The less steep the incline, the less the possible shift. Hence, we should expect a larger 'figural after-effect' when the previous distribution was narrow (hence falling away more sharply) than if it was broader. Therefore, although we could expect the after-effect to be at a greater distance than on Osgood & Heyer's original theory, it would necessarily be of an ever smaller magnitude.

III. The work of Marshall and Talbot

But is Osgood & Heyer's theory not founded on the work of Marshall & Talbot and, thus, on a sound neurophysiological basis? First, Marshall & Talbot's theory is vague and complicated. The neurophysiological observations they quote are highly ambiguous and consistent with interpretations quite different from those which these authors choose to make. Some of these interpretations are, furthermore, implausible. It is, for instance, difficult to accept, on *a priori* grounds, the notion that excitation from one receptor adds

up with that from the next on the cortex. This leads inevitably to a loss of information only some of which can be recovered by a repetition of the message, involving eye-movements, 'memory' and the like.

Secondly, the actual anatomical evidence referred to by Marshall & Talbot is against this notion of summation. Marshall & Talbot's statements regarding the evidence about dendritic overlap in the lateral geniculate body and the cortex are grossly misleading. Summarizing the work of Glees & Le Gros Clark on the lateral geniculate body in the cat and monkey, they say (p. 122) 'Apparently reciprocal overlap is less extensive in the monkey geniculate than in the cat.' Glees (1941, p. 438) expressly states: 'It is interesting to compare these results with the conditions which obtain in the monkey. In the latter it was found that the terminal boutons are extremely few in number, and since it was in no case possible to find more than one bouton related to each geniculate cell, it was concluded that in the lateral geniculate body of this animal there is no overlap of optic fibres'.

That by 'less extensive' they, presumably, do not mean 'absent' is made probable as they go on to assume that such overlap occurs in the human case. 'These unit paths then are related at each synaptic level by reciprocal dendritic overlap of increasing extent, amounting to perhaps 2° at the cortex (Clark, 1941, p. 135).' Again, Le Gros Clark, in the paper to which they refer, makes no statement which can, by any normal processes of reasoning, be interpreted to support this statement, or any part of it.

As this is the work they quote in support for their notion, their contention that there is summation of excitation between separate paths is made unlikely both on *a priori* grounds and by the excellent observations which they misrepresent.

Further, Marshall & Talbot say that (p. 123) 'if paired shocks of weak or moderate intensity are applied to the optic nerve of the nembutalized cat, the second shock produces an enhanced postsynaptic geniculate spike, during the first 10–30 msec. This mixture of supernormality, facilitation, recruitment or summation is succeeded by a longer period of depression or subnormality'. From this they extract a premise very important for their theory, namely, that (p. 127) 'a burst of impulses discharged from the retina would thus be relayed through the geniculate for 10–30 msec., then summation of subnormality would intervene to check the transmission'.

Now, there are three main conclusions to be drawn from the experiment with the two single-spaced electrical pulses. The first is that this supernormality-subnormality cycle occurs in the same way with the same time constants whether there are many pulses or no pulses between the initial and 'testing' pulse. This is Marshall & Talbot's conclusion and it is clearly fallacious unless they endow the initial impulse with special properties not shared by its successors. Otherwise, the pulses succeeding it must either add similar cycles or cancel anything preceding and begin anew. Either of these conclusions is not in harmony with the first one, on which the theory rests. It seems sufficiently obvious that radically different properties are generated by these two last assumptions. Also, we find entities like 'memory' and 'attention' (which themselves must be accounted for in physiological terms) playing a part in explanations which are intended to be physiological: 'The acuity is greatest because the formed edges attract the threshold of attention away from the interspace and background' (p. 146). 'This would allow for subvisible neural action leaving traces in memory' (p. 141).

Further, Marshall & Talbot are very reticent about numerical values and, especially,

about the extent of spread of excitation. On p. 135 they suggest tentatively that dendritic overlap could amount to 2° at the cortex. In fig. 6, p. 145, where they are attempting to explain some phenomena connected with the minimum separable, the spread from peak to end of distribution is 3′. Osgood seems to have 4° in mind.

Though a great deal of the explanation of acuity rests on eye movements, there is complete vagueness about the time over which information at the cortex is integrated. Assumptions about the time constants of this seem to be completely *ad hoc* and tailored to fit each particular case.

So far, the criticisms of Marshall & Talbot have mainly been on matters internal to their paper. However, since it was written, more evidence has come forward on this topic of eye movements and their alleged aid to acuity. The work of Ratliff (1952) and Riggs, Ratliff, Cornsweet & Cornsweet (1953) tends to show that acuity is hindered rather than helped by eye movements. 'It has also been demonstrated that the relatively large changes in the position of the eye as a result of the rapid tremor are a hindrance to monocular acuity' (Ratcliff, 1952).

In comparing the two theories, it is difficult to see how Osgood & Heyer's assumption that a contour is seen at the peak of a distribution could fit Marshall & Talbot's scheme. Also Osgood & Heyer reject Köhler & Wallach's three-dimensional projection. Marshall & Talbot do not. 'Alternatively, the "three-dimensional projection" may really exist in the higher level' (p. 158).

Again, Osgood & Heyer deal with contours, Marshall & Talbot with lines.

Thus there does not seem as much to choose between Köhler & Wallach and Osgood & Heyer in the way of physiological respectablity as the latter authors would have us believe.

If this criticism of the statistical theory is valid, it is hoped that Osgood & Heyer will amend it and, again, give us something which is both attractive and simple. Incidentally, there is often a tendency to think that, because one theory on the subject has had doubts cast on it, the other prevailing one must be correct. A criticism of Osgood & Heyer does not necessarily lend support to Köhler & Wallach.

IV. A tentative hypothesis

Köhler & Wallach's theory has been thought to be implausible mainly on neurophysiological grounds. However, one of their basic notions seems sound enough. They make displacements in phenomenal space not a shifting of the retinal projection on the cortex but a change in the functional interrelation of this projection. Such a supposition seems intrinsically likely because of both the extent and the distance over which figural after-effects operate. Instead of postulating the flow of direct currents, it is suggested that from each contour printed on the cortex emanates a wave front which travels at a constant speed. When this wave front reaches another contour, a pulse is generated. If then a pulse is also generated when this wave front begins to travel, the time between the two pulses will be proportionate to the distance between the contours. This notion has already been used in a theory of shape recognition (Deutsch, 1955). Here it was assumed that an impulse is propagated from each part of a contour at right angles to its tangent. We can now postulate, in a manner similar to Osgood & Heyer, some sort of refractory state of the conductivity of the tissue where a contour has been generating impulses which

it will have done in a particular direction. This will then have the effect of slowing down any travelling wave front; slowing it down, say, in proportion to the directional component which it shares with the wave front previously generated. This would, therefore, lead to overestimation of distance, depending on the orientation relative to the previous contour, of the two new points to be judged. There will also be a narrow fringe round the refractory area which conducts impulses more quickly, perhaps, as a result of a small amount of stimulation or rather as an effect of the abnormal state of the neighbouring tissue. This directional slowing down of two wave fronts (the deviations of whose speed give rise to expansion or contraction, as the case may be, of the interspace) could also, perhaps, explain some of the phenomena of motion and its after-effects, such as the waterfall illusion. The passage of a contour over a given area can be imagined to give rise to an acceleration of the propagation of a wave front in the direction of motion of the contour. The wave front, propagated in the direction reverse to that of the movement of the contour, would tend to be slowed up as it has to pass over an area over which a contour has most recently passed. There would, thus, be a difference in the rate of travel of the wave front propagated backwards and forwards.

When the movement of the contour ceases, this difference in the rates of travel will reverse itself. The forced speeding up in a particular direction will lead to a subsequent refractoriness to transit in the same direction. The wave front propagated in the reverse direction will no longer be passing over a space which has just been occupied by a contour, and the difference in speed of propagation, due to this factor, will swiftly reduce to vanishing point. Thus, impulses will be travelling more swiftly in one direction than in the other as they would be if the contours were actually moving in reverse. This, presumably, would generate at least some of the cues which would arise if the now stationary contours were really moving in the opposite direction.

The difficulty which both previous theories have over the median fissure seems to be more tractable on the present view. If an impulse which has reached the edge is conveyed from one hemisphere to the other sufficiently quickly and continues in the same direction, a functional unity is preserved.

The scheme outlined derives some support from recent neurophysiological observations such as those of Burns (1950, 1951), Burns & Grafstein (1952) and Motokawa (1951).

My thanks are due to Mr N. S. Sutherland for his discussion of the section on Osgood & Heyer and Mr J. Price for a discussion of some of the criticisms of Marshall & Talbot. I am also indebted to Dr Kay for his advice and encouragement at all stages of the preparation of this paper.

References

Burns, B. D. (1950). Some properties of the cat's isolated cerebral cortex. *J. Physiol.* **111**, 50–68.

Burns, B. D. (1951). Some properties of isolated cerebral cortex in the unanaesthetized cat. *J. Physiol.* **112**, 156–175.

Burns, B. D. & Grafstein, B. (1952). The function and structure of some neurones in the cat's cerebral cortex. *J. Physiol.* **118**, 412–33.

Clark, W. E. Le Gros (1941). Observations on the association fibre system of the visual cortex and the central representation of the retina. *J. Anat., Lond.*, **75**, 225–35.

Deutsch, J. A. (1955). A theory of shape recognition. *Brit. J. Psychol.* **46**, 30–7.

Fry, G. A. & Bartley, S. H. (1935). The effect of one border in the visual field upon the threshold of another. *Amer. J. Physiol.* **112**, 414–21.

Gibson, J. J. (1933). Adaptation, after-effect and contrast in the perception of curved lines. *J. Exp. Psychol.* **16**, 1–31.

Glees, P. (1941). The termination of optic fibres in the lateral geniculate body of the cat. *J. Anat., Lond.*, **75**, 434–40.

Glees, P. & Clark, W. E. Le Gros (1941). The termination of optic fibres in the lateral geniculate body of the monkey. *J. Anat., Lond.*, **75**, 434–40.

Hammer, E. R. (1949). Temporal factors in figural after-effects. *Amer. J. Psychol.* **62**, 337–54.

Hebb, D. O. (1949). *The Organization of Behaviour, A Neuropsychological Theory.* New York: Wiley.

Hull, C. L. (1952). *A Behaviour System.* New Haven: Yale University Press.

Köhler, W. & Wallach, H. (1944). Figural after-effects. *Proc. Amer. Phil. Soc.* **88**, 269–357.

Lashley, K. S., Chow, K. L. & Semmes (1951). An examination of the electrical field theory of cerebral integration. *Psychol. Rev.* **58**, 123–351.

Marshall, W. H. & Talbot, S. A. (1942). Recent evidence for neural mechanisms in vision leading to a general theory of sensory acuity. In H. Klüver (ed.), *Visual Mechanisms.* Lancaster, Pa; Cattell.

Motokawa, K. (1951). Propagation of retinal induction. *J. Neurophysiol.* **14**, 339–51.

Osgood, C. E. (1953). *Method and Theory in Experimental Psychology.* New York: Oxford University Press.

Osgood, C. E. & Heyer, A. W. (1951). A new interpretation of figural after-effects. *Psychol. Rev.* **59**, 98–118.

Polyak, S. L. (1941). *The Retina.* Chicago: Univ. Chicago Press.

Ratliff, F. (1952). The role of physiological nystagmus in monocular acuity. *J. Exp. Psychol.* **43**, 163–72.

Riggs, L. A., Ratliff, F. Cornsweet, J. C. & Cornsweet, T. N. (1953). Disappearance of a steadily fixated visual test object. *J. Opt. Soc. Amer.* **43**, 495–501.

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A THEORY OF PERCEPTUAL CONSTANCY

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A terminology is developed for perceptual constancy which permits useful mathematical treatment. This is illustrated by discussing a particular form of constancy for which the mathematics is particularly simple. Other cases which need more elaborate mathematics can be discussed in the same way.

This formulation of the constancy problem enables the empiricist approach to perception to be formulated more concretely. Instead of saying simply *that* learning is involved we are able to go some way towards saying *what* has to be learnt.

It is suggested that constancy can be established by conditioning each element of an 'equivalence class' of states of stimulation to the same group of responses. This idea needs much more elaboration and investigation (Taylor, 1956), and we do not maintain that it is the *only* possible mechanism.

Experimental results on protracted wearing of distorting spectacles are reported and discussed in the light of the above principles.

I. The problem

A central problem of perception theory is that of constancy. We are faced with the fact that objects in the external world stand in a one-many relation to patterns of stimulation of a perceiving organism, and patterns of stimulation stand in a many-one relation to perceptions. Effective perception depends on the result of compounding these relations yielding a relation between objects and perceptions which, at least within certain ranges, will be one-one. About the first of these relations a great deal is known; in fact, the essential features emerge from simple mechanical and optical considerations and from elementary peripheral physiology. On the other hand, extremely little is known about the second relation. One reason for this paucity of knowledge may well be that investigations of constancies in perception have been too *static*. Attention has been focused on investigating the constancies of normal perception, and too little use has been made of situations in which constancies can be destroyed and re-built by experimental manipulation. In the sequel we hope to illustrate this point by analysing a simple but illuminating example of such a procedure. This will lead us to the formulation of a terminology and of certain principles which seem to have a wide applicability in perception theory.

Consider a subject gazing at a scene in which there is no movement. Every shift in the position of his eyes, his head or his whole body will cause the image of the scene in front of him to fall on different parts of the retina and to change in orientation. Yet he sees the scene as stationary and constant in orientation. The mechanism which is responsible for the fact that each of these different patterns of stimulation gives rise to the same perception normally operates in an exceedingly complicated manner and involves a bewilderingly large number of variables, such as retinal stimulation, various forms of proprioceptive stimulation, previous experience of particular objects, and even emotional factors. To gain an insight into the nature of the process we will describe in detail a

situation in which the variables can be isolated and described with concrete accuracy. We are not suggesting that all aspects of perception are susceptible to quantitative treatment in the same way. What we claim is that certain aspects which can be described with this kind of precision are capable of elucidating principles which operate universally.

II. A particular constancy

Suppose, now, that our subject is monocular and that he is sitting, with his head erect, in front of a blackboard on which rectangular co-ordinates have been drawn with the x-axis horizontal and the origin in front of the subject's eye. Let $L(\theta)$ stand for a straight line drawn from the origin making an angle of θ with the x-axis, as in Fig. 1. The retinal image

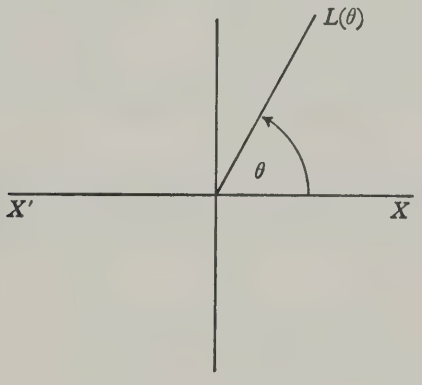

Fig. 1. The objective field.

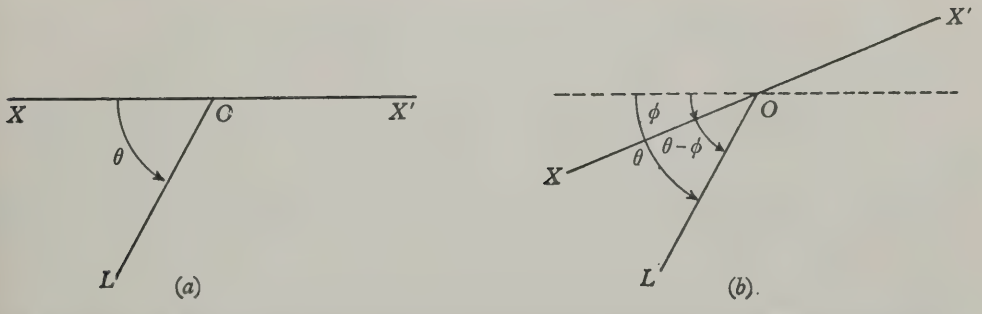

Fig. 2. The retinal field when the head is (a) erect, (b) rotated about the optic axis through the angle ϕ.

of this figure, as inverted by the optical system of the eye, is shown in Fig. 2a, where OX is the image of the x-axis and OL the image of $L(\theta)$. A retinal reference frame is defined by supposing OX to be permanently ruled on the retina, so that it coincides with the image of the x-axis only when the head is in this position. On the other hand, the line OL will always be taken to be the actual image of the line $L(\theta)$. Thus if the subject tilts his head by rotating it about the optic axis through an angle ϕ, the line OX is carried with the retina while OL remains fixed in space. The situation is represented by Fig. 2b, where it will be seen that the image of $L(\theta)$, viz. OL, now makes an angle $(\theta-\phi)$ with the retinal reference line OX.

As the subject rotates his head in this manner, $L(\theta)$ does not appear to move, although its image is sweeping across a large area of the retina. Quite obviously this constancy cannot be accounted for by retinal variables alone; the proprioception corresponding to the rotation of the head must play a part. The advantage of choosing this simple situation is that we can specify the relevant states of the retinal cells and the proprioceptors by two numbers ψ and ϕ. The symbols (ψ, ϕ) will denote the state of stimulation of the subject when (i) the line OL on the retina makes an angle ψ with OX and (ii) the head is inclined at an angle ϕ. In the situation represented in Fig. 2*b*, (ψ, ϕ) becomes $(\theta-\phi, \phi)$.

We can now write down explicitly the relations referred to in the first paragraph:

$L(\theta)$ can give rise to any one of the stimulus patterns $(\theta-\phi_1, \phi_1)$, $(\theta-\phi_2, \phi_2)$,..., etc. (Relation 1)

Any one of these stimulus patterns gives rise to the perception of a line in position $L(\theta)$. (Relation 2)

It is shown in Note A at the end of the paper that relation 2 can be written in the form

The stimulus pattern (ψ, ϕ) gives rise to the perception of a line in position $L(\psi+\phi)$ (Relation 2′)

The property of giving rise to the same perception defines an equivalence amongststimulus states which we refer to as a Perceptual Equivalence Relation. This relations tructures the set of stimulus states into *equivalence classes*. We will use the symbol $E_1(\theta)$ to denote the equivalence class

$$(\theta-\phi_1, \phi_1) \equiv (\theta-\phi_2, \phi_2) \equiv \ldots.$$

We note that two stimulus states (ψ_1, ϕ_1) and (ψ_2, ϕ_2) belong to the same equivalence class $E_1(\theta)$ if, and only if, $\psi_1+\phi_1=\psi_2+\phi_2(=\theta)$.

III. Role of experience in perception

The terminology we have developed allows us to pose in a very precise manner one of the main questions that divide nativists from empiricists. Can the subdivision of the set of stimulus states into equivalence classes be accounted for in terms of the anatomical structure of the nervous system, or does it depend on the experience of the organism? We are able to answer this question categorically by reference to experiments in which perceptual equivalence relations are changed in a predictable way. The existence of a process by which the existing equivalence classes can be disrupted and replaced by a different set provides a strong presumption that the original ones came into being by the same process and are not innate.

The constancy discussed in the previous section can be altered by the protracted wearing of a pair of spectacles which, by means of an arrangement of mirrors or total reflecting prisms, reflects the visual world in one plane. For example, prisms with their bases in the horizontal plane of the head will cause the world to be seen upside down but will not disturb left-right relations, while prisms with their bases parallel to the medial plane will reverse left-right but not up-down. Unlike the system of lenses used by Stratton (which reverse in both these directions simultaneously) an arrangement of this type produces an instability of the perceptual field when the head is tilted. Fig. 3 shows the effect of reflecting the perceptual world by means of prisms with bases in the horizontal plane. With the head erect (Fig. 3*a*) the world is seen upside down. When the head is

tilted through the angle ϕ (Fig. 3*b*) the image of $L(\theta)$ makes an angle $\phi-\theta$ with OX, thus giving rise to the stimulus state $(\phi-\theta, \phi)$ which belongs to the equivalence class $E_1(\phi-\theta+\phi)=E_1(-\theta+2\phi)$. Thus a rotation of the head through ϕ causes an apparent rotation of the external world through 2ϕ. The description of the *direction* in which this apparent movement takes place needs some care since the following two statements are not always equivalent:

(*P*) The world appears to rotate in the same direction as the head but through twice the angle.

(*Q*) When the head moves through an angle ϕ the subject sees the same movement as he would were his head kept stationary and the line $L(\theta)$ rotated through an angle -2ϕ to the position $L(\theta-2\phi)$.

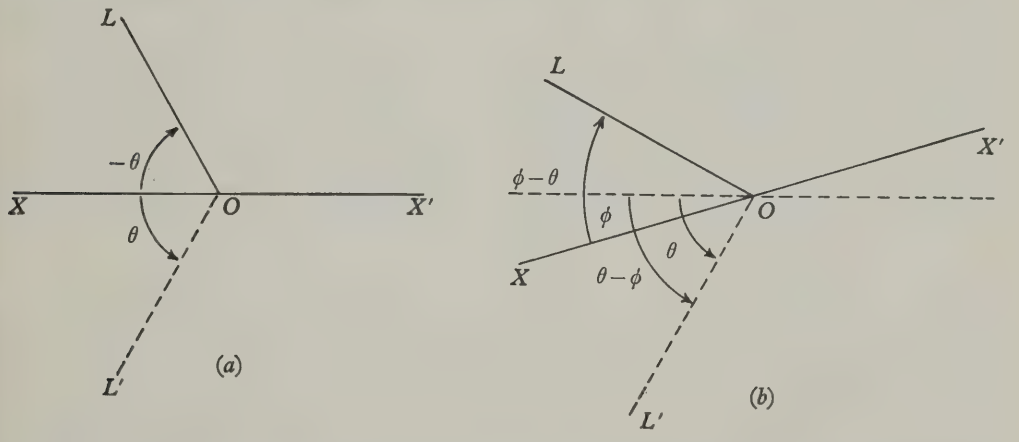

Fig. 3. The retinal field with inverting spectacles, when the head is (*a*) erect, (*b*) rotated through the angle ϕ. The broken line OL' shows where the image of $L(\theta)$ would lie without spectacles.

The need to make the distinction between these two modes of description will become apparent when we discuss the effects of protracted wearing of spectacles.

In experiments conducted by Erismann and Kohler and by the present authors (Kohler, 1951, 1951*a*; Rakoff, 1953) spectacles of this kind* have been worn for periods ranging from a few days to 10 weeks. All subjects who have worn the spectacles for sufficiently long periods report that eventually the instability induced by the spectacles disappears—rotation of the head no longer induces any apparent movement. *Nearly* all subjects also eventually report that the perceived world no longer appears to be inverted. Our main concern in the following discussion will be with the first of these phenomena, though we shall have to refer to the second in assessing experimental results.

A subject who perceives the world as stable while wearing the spectacles has established a new set of equivalence classes (i.e. new constancy relations) which may be represented by

$$E_2(\theta)=\{(\phi_1-\theta, \phi_1)\equiv(\phi_2-\theta, \phi_2)\ldots\}.$$

This time the relation of invariance takes the form

$$(\psi_1, \phi_1)\equiv(\psi_2, \phi_2) \quad \text{if, and only if} \quad (\phi_1-\psi_1)=(\phi_2-\psi_2).$$

* What we have to say applies *mutatis mutandis* to left-right reversing as well as to up-down reversing spectacles. Hence we shall not distinguish between these two types in the following discussion.

This last condition is satisfied by any subject who sees the world as stable, *irrespective of whether he describes the world as inverted or not.* In both cases $(\phi_1 - \theta, \phi_1)$ and $(\phi_2 - \theta, \phi_2)$ will give rise to the same perception—the difference is that in the one case this will be a perception of a line in the position $L(-\theta)$ and in the other case a line in position $L(\theta)$.

We now turn to the deduction of what must happen when the spectacles are removed after adaptation. Reference to Table 1 shows that if the equivalence classes E_2 are operant, then the subject must see the world tilt through an angle 2ϕ when he tilts his head through an angle ϕ. Moreover, he will see the line tilt in the same direction as

Table 1. *States of stimulation (rows* 1 *and* 4*) as determined by (a) direction of the object line, (b) position of head, and (c) whether spectacles are worn or not; and the resulting perception (rows* 2, 3, 5 *and* 6*) as determined by (d) the equivalence relation that is in operation at the time*

Equivalence relation in operation	Entries in following columns describe	Line at $L(\theta)$, head erect	Line at $L(\theta)$, head at ϕ	Line at $L(\theta - 2\phi)$, head erect
		Without spectacles		
—	State of stimulation	$(\theta, 0)$	$(\theta - \phi, \phi)$	$(\theta - 2\phi, 0)$
$E_1(\psi, \phi)$	Perception	$\theta + 0 = \theta$	$\theta - \phi + \phi = \theta$	$\theta - 2\phi + 0 = \theta - 2\phi$
$E_2(\psi, \phi)$	Perception	$0 - \theta = -\theta$	$\phi - (\theta - \phi) = -\theta + 2\phi$	$0 - (\theta - 2\phi) = -\theta + 2\phi$
		With spectacles		
—	State of stimulation	$(-\theta, 0)$	$(\phi - \theta, \phi)$	$(2\phi - \theta, 0)$
$E_1(\psi, \phi)$	Perception	$-\theta + 0 = -\theta$	$\phi - \theta + \phi = -\theta + 2\phi$	$2\phi - \theta + 0 = -\theta + 2\phi$
$E_2(\psi, \phi)$	Perception	$0 - (-\theta) = \theta$	$\phi - (\phi - \theta) = \theta$	$0 - (2\phi - \theta) = \theta - 2\phi$

Note that the third row, which represents the after-effects of adaptation to the spectacles, is the same as the fifth row, representing the first effects of wearing the spectacles.

that which would be produced by tilting the line in the opposite direction with the head kept fixed. Thus if we use the form of description referred to above as (Q), the *after-effect instability is exactly the same as that observed on first putting on the spectacles.* If, in addition, the subject has adapted in such a way that he not only sees the world as *stable* through the spectacles but also as *non-inverted*, then he will see the world as inverted when he removes the spectacles, and he will describe the apparent movement produced by tilting his head as being in the same direction as the movement of his head. Thus the form of description (P) would also apply. However, if he sees the world as stable with the spectacles, but *continues to see it as inverted*, description (Q) but not description (P) would apply to the after-effect.

The experimental verification of these deductions is incomplete. At the time they were made* they were apparently in flat contradiction to the only evidence available. Kohler (1951*a*) had reported that after removing the spectacles the subject saw a tilt which was in the opposite direction to the movement of the head. No test in the form (Q) was applied. In our own experiments we had sacrificed the opportunity of making this observation by adapting our experimental design to answer certain other questions, having, at that time, no reason to be dissatisfied with Kohler's result. However, a re-examination of Kohler's observation shows two features which indicate that it might be quite irrelevant in the present context. First, he saw tilting only when rapid movements of the head were made and these movements were not functionally related to the extent of the head movement—

* A preliminary draft of this paper was circulated in duplicated form in January 1954.

they were more similar to the flick instability which can be observed by the reader if he makes a rapid jerking movement of his head. Secondly, the observation was not immediately made after removal of the spectacles. For these reasons we consider that they cannot be regarded as simple after-effects of the type under discussion.

A later experiment conducted at Innsbruck in the spring of 1954 yielded results which were in accordance with our prediction, though they do not completely verify it. In this experiment the subject wore his spectacles for 71 days. He reported* stability under head rotation but continued to report reversed vision to the end of the experiment. On removing the spectacles he reported that the world tilted when he tilted his head, that the tilt was functionally connected to the extent of head tilting and that it was in the direction opposite to that of the head movement. This is what our deductions predict for a subject whose vision with the spectacles remains reversed. No test in the (Q) form was applied.

Further experiments on this point have been planned. It should be noted that apart from its importance in testing the validity of the present approach (which must collapse unless this prediction is true) the existence of an after-effect which is identical with the original effect of putting on the spectacles is itself interesting. For this reason we have adduced an *ad hoc* argument which reaches the same conclusion in a different and less satisfactory way. This will be found in Note B at the end of the paper.

IV. Some other spectacle experiments

The process of altering equivalence classes and so constancy relations is further illustrated by the following experiments, the details of which will be reported elsewhere.

(1) In an experiment conducted by Taylor, Papert and Schermann in July–August 1953 the subject (Papert) wore left-right reversing spectacles for 8 hr. a day. In this case the relevant stimulus states have to be specified by three variables, viz. the retinal stimulation, the proprioceptive stimulation, and a third variable which takes on the values 0 and 1 corresponding to 'spectacles off' and 'spectacles on' respectively. It was found that stability could be achieved both with and without the spectacles. In the restricted situation described in §II the stimulus states may be represented by the triplet (ψ, ϕ, x), where x is the parameter corresponding to spectacles on or off. The equivalence relation achieved during the experiment can be written

$$(\psi_1, \phi_1, x_1) \equiv (\psi_2, \phi_2, x_2)$$

if, and only if, $$(\phi_1 + (1-x_1)\psi_1 - x_1\psi_1) = (\phi_2 + (1-x_2)\psi_2 - x_2\psi_2).$$

Incidentally, it was also found that 'correct' perception was reported both with and without the spectacles.

(2) An experiment conducted by Papert in Cambridge during January to February 1955 provides a number of examples of the alteration of constancies by the use of spectacles consisting of 45° prisms arranged as in Fig. 4. The effects of such prisms are rather complex, but two fairly simple examples can be discussed without going into detail.

(a) A suitable tilting movement of the head will cause the field to move upwards when the head goes to the left and downwards when the head moves to the right. The extent of

* Personal communication by I. Kohler.

this movement is not uniform in different parts of the field, and so a complicated distortion is induced. However, this aspect will be ignored here. After 5 weeks of life in the spectacles, this apparent movement had considerably decreased, though it still remained in certain circumstances. When the spectacles were removed an after-effect movement was observed. This was in the direction opposite to that originally induced by the spectacles, but considerably less in extent and not always very clear. However, when the test was carried out in a dark room with only a luminous point visible, the extent of the after-effect movement was approximately equal to that of the original effect of the spectacles, though in the opposite direction. We suggest that when the full field is viewed with full illumination there are numerous components of the situation which are able to act like the parameter x in the previous example, so that the 'spectacle' habits have to compete with 'pre-spectacle' habits evoked by these cues. In the dark no such situation obtains and the most recently established set of habits operates unhampered. (This observation may be relevant to the problem of after-effects with reversing spectacles referred to in the previous section.)

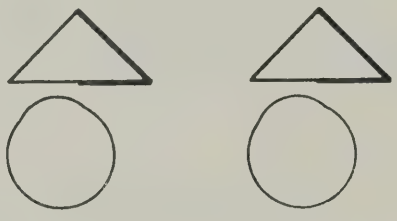

Fig. 4. 45° prism spectacles. Heavy lines indicate blacked-out surfaces.

The fact that the after-effect movement should in this case be in the direction opposite to the original movement is easily deduced by drawing up a table similar to Table 1. (See also Note C.)

(*b*) When the spectacles are first worn there is a very marked compression effect—as objects move to the right of the visual field they are increasingly shortened in their horizontal dimensions. Thus a square in a suitable position will appear to be a rectangle with its vertical side longer than its horizontal side. If it is rotated it appears to change its shape, the apparently longer side becoming apparently shorter and vice versa. After adaptation to the spectacles a reverse effect is seen when they are removed—a horizontal line of length 5 in. will appear to be equal in length to a suitably placed vertical line of length 6 in. Here we have a new constancy relation set up which differs from those previously discussed in that it does not depend essentially on a proprioceptive factor.

The equivalence relation for length of a line segment must be written in the form

$$(x_1, f(\chi_1)) \equiv (x_2, f(\chi_2)),$$

where x_1 is the length of the retinal image of the segment, χ_1 is the angle it makes with the retinal axis and f is a suitable function.

This case illustrates the fact that the kind of analysis used above applies also when the variables are not simply 'retinal stimulation' and 'proprioceptive stimulation'. However, the mathematics involved is somewhat more complex and will be published fully elsewhere.

V. Learning to see

We now have to pose the question: by what process do the equivalence classes come into being? In our discussion of the equivalence classes underlying the constancy of the visual field when the head is tilted we identified a mathematical invariant—$\psi+\phi$ in the case of a normal subject, $\phi-\psi$ in the case of a subject adapted to reversing spectacles. Our problem is to discover a physical invariant corresponding to this mathematical construct which is responsible for bringing together the various stimulation states which will eventually form an equivalence class. The nativist approach leads one to seek this in the structure of the pattern of stimulation or of the brain process aroused by it. Examples of this approach are the models which have been proposed by the Gestalt 'field theory' psychologists, by McCulloch (1951) and by Deutsch (1955). Each of these theories postulates a mechanism, anatomically determined, which will actually reduce all the members of the same equivalence class to a common canonical form. It seems clear from the spectacle experiments that while such mechanisms may exist, they cannot provide the answer to the problem which they are designed to solve. Our hypothesis is that the physical invariant underlying the equivalence classes is not an intrinsic property of the stimulation states or of the anatomically determined brain processes set up by them, but is to be found in the set of responses with which these stimulus states have been associated through the individual's commerce with the external world.

Consider, for example, our original subject with his reversing spectacles. Suppose that he sits in front of his blackboard and tries to trace with his fingers the various lines $L(\theta)$. If we denote by $R(\theta)$ the action of moving the finger in the direction of the line $L(\theta)$, we find that his initial tendency is to respond to $L(\theta)$ with the movement $R(-\theta)$. This will not be successful and he will try various other responses until eventually he hits on the correct one $R(\theta)$. After some practice he will set up a conditioned response which may be represented by

$$(-\theta, 0) \rightarrow R(\theta).$$

He now tilts his head to the position ϕ_1. The stimulus pattern will change from $(-\theta, 0)$ to $(\phi_1-\theta, \phi_1)$, and he will again make a series of incorrect responses until he finds and practises the correct one which is again $R(\theta)$. Eventually the patterns of behaviour

$$(\phi-\theta, \phi) \rightarrow R(\theta)$$

will be established for all values of ϕ, and so the set of stimulus states of the form $(\phi-\theta, \phi)$ will have been associated *through* the common response $R(\theta)$. Moreover, this association would be reinforced by *any* other mode of behaviour; at least on the overwhelming majority of occasions the response appropriate to $(\phi_1-\theta, \phi_1)$ is also appropriate to $(\phi_2-\theta, \phi_2)$ and vice versa. Our hypothesis is that the constancy is due to the accumulation of associations of this type and its extent a function of the extent to which the association is invariable.

The last remark may be illustrated by reference to the vexed question of the constancy of a rectangle (say a matchbox) which moves in such a way as to make the retinal image increasingly unrectangular. Whatever the angle of view of the matchbox, the attempt to lift it requires that the fingers be held the same distance apart, holding it in the palm requires the same degree of closure of the hand, etc. However, the constancy of shape is not as absolute in this case as in the case of vertical lines which remain vertical when the head is

tilted. One reason for this may be that while there is a common element to the responses made to the matchbox in various positions, there is also a difference which is more appreciable than the difference which exists in the other case.

Note A. Suppose the line which gives rise to the stimulus state (ψ, ϕ) is $L(\theta)$. We know from relation 1 that $L(\theta)$ gives rise to stimulus states of the form $(\theta - \phi, \phi)$. Thus $(\psi, \phi) = (\theta - \phi, \phi)$ i.e. $\psi = \theta - \phi$ or $\theta = \psi + \phi$.

Note B. Suppose (if possible) that X is a subject who has *completely* adapted to the spectacles (say by wearing them for 50 years). Then the effect induced by putting another pair of similar spectacles on X *over* his experimental pair will be the same as that induced by putting a pair of spectacles on to a fresh subject Y. Thus X + two spectacles = Y + spectacles. But simple optical considerations show that two prisms in tandem are essentially the same as no prisms, i.e. this system will produce neither inversion nor instability of the type under discussion. In other words X + no spectacles = Y + spectacles. Q.E.D.

Note C. The argument of Note B does not apply to this case, since two prisms of this type arranged in tandem will *not* be equivalent to the situation of no spectacles. To annul the effect of one of these prisms it is necessary to have a prism of opposite orientation, and the perceptual effect of such a system is to produce instability movements in the opposite direction.

It should be noted that while arguments of this form are often useful they must be used with caution lest the blanket assumption of *complete* adaptation beg the question.

References

Deutsch, J. A. (1955). A theory of shape recognition. *Brit. J. Psychol.* **46**, 30–7.

Kohler, I. (1951). *Über Aufbau und Wandlungen der Wahrnehmungswelt.* Vienna: Österreichische Akademie der Wissenschaften.

Kohler, I. (1951*a*). *Protocol of Experiments on Wearing Reversing Spectacles.* In Institute für experimentelle Psychologie, University of Innsbruck.

McCulloch, W. S. (1951). Why the mind is in the head. In L. A. Jeffress (ed.), *Cerebral Mechanisms in Behavior.* New York: Wiley.

Papert, S. (1956). Mathematical Appendix to Taylor (1956).

Rakoff, L. (1953). An investigation into the effects of differential conditioning to up-down reversal of the visual field. Thesis in the Library of the University of Cape Town.

Taylor, J. G. (1956). The behavioural basis of perception. (In preparation.)

(*Manuscript received* 4 *December* 1954)

PUBLICATIONS RECENTLY RECEIVED

Human Personality and its Survival of Bodily Death. By Frederic W. H. Myers. New York and London. 1954. 2 vols. 105*s*.

This monumental work, first published in 1903, has had great influence on the development of psychical research. It has not been much read by psychologists. One reason for this is made clear in the opening of Myers's Introduction, in which he speaks of his object as that of inquiry into whether there is any element of personality which survives death. Although this is certainly a momentous question, it is one in which experimental psychology has not yet interested itself, and, to many experimental psychologists, it seems certain that it never will. It is, nevertheless, a question in which William James was interested, and the sympathetic introduction to the present reprint by Gardner Murphy is evidence that some modern experimental psychologists share this interest.

While this was Myers's main intellectual problem, he ranged widely over the field of psychical research. He was one of the initiators of the experimental study of telepathy, although experimental work in this field has carried the subject much further than it was in Myers's time. Although his training was in the Classics and not in the Sciences, Myers had read widely in the psychology of his time, particularly in psychopathology. He is said to have been the first to introduce the early work of Breuer and Freud to the English-speaking world and was interested in the work of Charcot when few other people were. His extensive glossary of psychological and psychopathological terms gives evidence of the width of his reading in these subjects.

But if he was successful in learning about the science of psychology as it was in his time, Myers was perhaps less successful in mastering the ways of thought characteristic of science. This does not mean that he was not thorough in his collection of data and rigid in his criteria of evidence. His voluminous case histories and appendices have been a mine from which other students have dug out facts ever since, and no one could have been more careful than Myers not to draw conclusions beyond those justified by his data. What seems unfamiliar to the scientific reader is the lack of method in Myers's conceptualization and his tendency to use emotive language. For example, in putting forward the very prosaic idea that the essential element of poetic excitation lies in scarcely perceptible changes of tension in the muscles of the throat, he goes on to say that they so 'flood the flats of common consciousness as with the earthquake-wave of an unfathomed sea' (vol. I, p. 102). For better or worse, that is how scientists have been trained not to think, and references to 'unfathomed seas' and 'subliminal uprushes' are inclined to create a prejudice in the mind of the scientific reader which may not be altogether reasonable.

Myers was not a scientific psychologist but a classicist who knew a great deal about scientific psychology. We must bear with the fact that he thought like a classicist and appreciate that he applied his thinking to a number of very interesting problems which have been relatively neglected by those with a more rigorously scientific cast of mind. His book deserves wide reading. There may be some superfluous ornaments to the thought, but the matter is important and well documented. We may decide that nothing can survive bodily death, but the grounds for doing so must be somewhat uncertain until we have read Myers's case for regarding survival as a possibility. R. H. THOULESS

Über Geschehensgestaltungen in der Auffassung von Filmen durch Kinder. By I. Gerhartz-Franck. Leipzig: Johann Ambrosius Barth-Verlag. 1955. Pp. iv + 124. DM. 7.20.

This slim volume constitutes the third in a series edited by Prof. Gottschaldt (Berlin) on child development. The present research investigates the way in which children of different age and intelligence levels perceive and comprehend films. The orientation underlying the study is that of Gestalt psychology. The study is an excellent one; it combines the ingenuity and freshness of Piaget's approach with the rigour of experimental design. The author showed a short film of the Beatrix Potter story of the town mouse and the country mouse to groups of children ranging from 6 to 15 years of age. Immediately after seeing the film, the children were asked to describe it. Interesting age differences occurred; the 6-year-olds, for example, disregarded the structure of the film, the psychological problems portrayed and simply enumerated a number of scenes, giving prominence to scenes describing activities with which they were familiar, such as washing, eating, etc. With increasing age, more details were remembered and a more

chronologically correct picture presented. It was only at about 12 years of age that major attempts at interpretation were made. It is of particular interest that, after the age of 11, the wealth of detail reported decreased with increasing age—comprehension of the structure of the film led to a decrease in the reporting of minor incidents which are not closely linked to the central theme.

An experimental variation of the film, made by interspersing an irrelevant film sequence, led to greater clarification of the way different age groups structured their perception of the film. Other techniques used were those of delayed recall and of recall aided by presenting the child with stills of the major scenes. A second presentation of the film led to descriptions of the film by the 6-year-olds in a manner characteristic of the 8-year-olds, and similarly the presentation of the 8-year-olds appeared more mature. The author points out implications for the use of films as a teaching medium for young children.

HILDE T. HIMMELWEIT

The Journal of Analytical Psychology. London: Tavistock Publications, Ltd. 1955. Vol. I, no. 1. Pp. 108. 21*s.* per volume of two issues.

It is an excellent plan that a new journal of psychology should have been started under the independent sponsorship of the Society of Analytical Psychology, with the collaboration of C. G. Jung, and it is to be hoped that the example will be followed elsewhere in Britain. This issue includes articles by J. Layard, R. Hobson, R. Moody, L. Stein and A. Allenby, with an editorial note, a short communication by A. Storr and reviews of Jung's works.

Although the journal will not please the statistical and experimental workers on clinical psychology, personality and social psychology, it will appeal strongly to those for whom Jungian theories and insights are important, and it is satisfactory that their ideas should have an independent channel of communication in Britain.

R. W. PICKFORD

Contributi del Laboratorio di Psicologia, Series XVIII and XIX. (*Pubblicazioni dell' Università Cattolica del S. Cuore*, Vols. XLVIII and XLIX.) Milan: Vita e Pensiero. No date. L. 7000 each.

These volumes contain reports of some of the work carried out between 1952 and 1954 in the highly reputed Laboratory of Experimental Psychology at the Catholic University of the Sacred Heart, Milan, directed by Fr. Agostino Gemelli. Although most of the papers are in Italian, some are in English, French or German.

A paper by Gemelli on 'The perplexities of modern psychology' expounds the view of psychology which is presumably prevalent in his Department. He has unfavourable things to say about idealist philosophers, including such revered figures as Croce and Gentile, who shun scientific psychology as an upstart of positivist or materialist parentage and offer their own bad psychology as a substitute. At the same time, he takes to task Wundtian atomism, psychoanalysis and the testing movement, while accepting their partial validity, on the grounds that they do not study the 'whole man'. Those psychologists who trespass on philosophical territory, usually by offering materialist solutions for the mind-body problem, are harshly dealt with, and his strictures against the Cartesian tradition are severe. He insists on the psychophysical unity of the human organism and draws sustenance for this position from the writings of Aristotle, Aquinas, Rosmini and the Gestalt psychologists, although his vitalist leanings would surely not have the concurrence of the last-named.

The other contributions testify to Gemelli's own versatility, to the range of interests represented among his colleagues and to their eagerness to combine high scientific standards with Catholic orthodoxy. The principal fields are industrial psychology, personality tests, sensory thresholds, perception, reaction-times and the spectrographic analysis of speech. Zavalloni produces an interesting synthesis of the Rogerian technique of psychotherapy and the Catholic conception of the act of choice, and adds to current attempts to measure changes under therapy objectively. A great deal of acquaintance with recent American and German work is shown, and some of the papers are about adaptations of well-known American tests. Isolated ideas and devices from the U.S.A. have been taken over, but there are few signs of an interest in the wider methodological and theoretical issues with which these are bound up. On the whole, the point of view is rather like one that was until recent years quite prevalent in Great Britain and still has its adherents—a conception of psychology as a collection of disparate problems and a disinclination to search for any but the most elementary unifying principles, a highly accomplished pre-

occupation with experimental techniques combined with an indifference to sophisticated theoretical techniques.

The price of these books—about £3. 10*s.* each at the current rate of exchange—is unlikely to turn them into best-sellers among English-speaking psychologists. But they form a worthwhile acquisition for libraries and a means of keeping in touch with the work of one of the most important continental centres of experimental psychology.

D. E. BERLYNE

Conceptions of Modern Psychiatry. By HARRY STACK SULLIVAN. London: Tavistock Publications. 1955. Pp. xiii+298. 32*s.* 6*d.*

Tavistock Publications are to be commended for making available to British readers this work, hitherto unpublished in this country. It contains the five lectures that Sullivan delivered in 1939 as the William Alanson White Memorial Lectures, with a further chapter on Sullivan's theories by Patrick Mullahy. In these lectures are to be found a restatement of Sullivan's standpoint and views, although he himself felt that they were an inadequate account of his position.

For Sullivan, psychiatry is the study of interpersonal relationships, and the ultimate causes of mental disorder, he believed, are to be sought always in the social order itself. Starting with his basic conceptions, he goes on in this work to give a brief but, nevertheless, comprehensive account of the emotional development of the child. Here we encounter Sullivan's ideas on empathy, his concept of the self dynamism and his concept of anxiety. He uses some unfamiliar terms to expound his ideas, but his basic formulation, the relating back of all emotional and mental illness to the field of interpersonal relations, is never obscured. His theories, moreover, are rooted in biology, and he stresses the continuity between the biological and the cultural.

He is perhaps at his most fascinating in discussing schizophrenia, a condition which he divorces entirely from the organic condition dementia praecox. He discusses schizophrenia from a truly dynamic standpoint as a 'disorder of living', and contrasts the standard Kraepelinian formulation with his own. If only for this essay alone, this book is worth reading. Even the most conservative cannot fail to be impressed by the author's grasp of the patient's mode of thinking and the way he seems to understand the schizophrenic's inner situation. As might be expected, physical methods of treatment are frowned on by the author—they reduce the patient's capacity for being human, he writes—and he makes his case as convincingly as anyone in this field. This work will repay study and stands as a fitting memorial to a great American psychiatrist.

H. I. CLAPHAM

The Psychiatric Interview. By HARRY STACK SULLIVAN. London: Tavistock Publications. 1955. Pp. ix+246. 28*s.*

This is the second posthumousp ublication of Sullivan's work by the William Alanson White Psychiatric Foundation. The book is based on recordings and notes of a lecture series given by Sullivan, and to its advantage much of the lecturer's pithy, racy style is retained.

The title might suggest that this is for specialist consumption only. However, psychiatry to Sullivan is the field of study of interpersonal relations and its core is the interview situation, a special instance of interpersonal relations. Thus Sullivan's work is directed not only to the specialist in mental or nervous disorders but to all those who can be considered experts in interpersonal relations. A subtitle specifies social worker, personnel manager and counsellor. The reviewer would have liked to see the list include psychologist, even though the criterion of expertness in human relations might prove embarrassing. Psychology tends to make us only too aware of the technical faults of interviewing, but it is unfortunate that we should just accept the deficiencies of the procedure and seek other more valid techniques, rather than investigate ways in which the interview could be improved as a device.

Sullivan's work is really a highly personal account of his particular interviewing skill, but his analysis and description of the interpersonal processes between interviewer and interviewee stress the importance of a 'two-person psychology' of interviews. This idea alone could well provide the basis for a re-examination of the scientific status of interviewing. Sullivan's minute analysis of interpersonal processes seems to be scientifically fruitful as well as an aid to professional interviewing.

Thus the book could be read to advantage by psychologists other than clinical psychologists. Clinical publications are already appearing in which Sullivanian ideas of interpersonal relationships are being examined experimentally. This book is recommended as a readable introduction to Sullivan's philosophy as well as an interesting analysis of a professional skill which is too often disparaged or concealed as 'intuition'.

R. M. MOWBRAY

The Interpersonal Theory of Psychiatry. By HARRY STACK SULLIVAN. London: Tavistock Publications. 1955. Pp. xviii + 393. 35*s*.

This book is one of the trilogy of Sullivan's works recently published in Britain. It is a compilation of his lectures and unpublished writings, amplified at certain necessary points by editorial comments referring to the author's earlier work. The result is a highly significant exposition of the modern psychodynamic approach of the Washington–Baltimore school of psychiatry.

A wide variety of topics is dealt with in this volume, which is divided into four parts. Part I deals with basic concepts and, in particular, with Sullivan's developmental approach to psychiatry. In his view the most worth-while understanding of sick and healthy human beings is derived from a detailed consideration as to how 'from birth onward a very gifted animal becomes a person'. Sullivan maintains that grasp of the significance of anxiety is fundamental to the psychiatrist's undertaking of valid therapeutic work with patients. While he does not embrace the semantics of Freudian ego-psychology, his system demands the learning of a private language. He pays less attention to the minutiae of psycho-sexual development than to the concept of interpersonal interaction taking place in a social field.

Part II represents the bulk of the book, and here the author considers the emotional significance of the various developmental epochs from early infancy to late adolescence in a systematic and detailed fashion. The orientation is a broad one, attempts being made where possible to correlate psychological findings with neuro-physiological data; for instance, when discussing the infant's psychological differentiation of his own body, Sullivan stresses the importance of the manual-oral co-ordination in the development of body libido. This emphasis upon 'two-zone co-ordination' is one of the keystones of his developmental theory. Finally, in this section, the author states his views on emotional maturity and on a sense of self-respect as a necessary condition of maturity.

In Part III the most arresting contribution concerns Sullivan's attitude to schizophrenia. Here he regards schizophrenia as a defensive position or way of life adopted by the patient who is unable to deal otherwise with traumatic situations ('not-me experiences') that arouse intolerable anxiety. The idea that schizophrenia represents 'a way of life' does not really amount to a theory of schizophrenia; it is rather a viewpoint which chooses to disregard such possible factors as genetic predisposition or immaturity of neural organization.

Part IV is devoted to a consideration of the relevance of social and cultural factors to psychiatry as a whole. Here, Sullivan moves away from psychiatry as a clinical speciality towards a psychiatry which purports, a little grandiosely, to become 'a science of living' and which is more intimately linked with the social sciences. In this connexion, he points out how this 'science of living' imposes a duty upon both psychotherapist and patient to foresee, if possible, the near future as well as the past.

This book is not only of inestimable value to the practising psychoanalyst but also to any psychiatrist who would achieve a dynamic understanding of his patients. Despite occasional obscurities of expression the book provides a highly stimulating account of Sullivan's humanism and of his weighty theoretical contributions to psychiatry.

A. BALFOUR SCLARE

Fragments of an Analysis with Freud. By JOSEPH WORTIS. New York: Simon and Schuster; London: Collet's Holdings Ltd. 1954. Pp. xii + 208. 18*s*.

This book contains a diary of what transpired during the course of a 'didactic analysis' which Dr Wortis underwent with Freud during the four months October 1934 to January 1935 inclusive. He assures us that, whilst he has bowdlerized the account, he has not distorted it; and the chief reason which he gives for the bowdlerization is that he has 'no wish to achieve distinction by posing naked in the street'. On the assumption that this consideration is to be taken at face value, it would be difficult to explain his wishing to publish the diary at all, were it not for the fact that it is obviously intended to be a study of Freud rather than of himself. The analysis was undertaken while Dr Wortis was in Vienna as a research scholar studying sex problems; and evidently he regarded the circumstances as providing a favourable opportunity for a Cook's tour of psychoanalysis. What induced Freud to accept him as an analysand for a prescribed period of four months is only one of the many puzzling aspects of the account; for the surface of the unconscious could barely be scratched in such a short period. The term 'didactic analysis' is also puzzling, since to be didactic is foreign to the aims of a personal analysis. However, if the account provides a fair picture of what occurred during sessions, Freud must have been the least orthodox of all analysts; and the sessions are represented as consisting largely of theoretical discussions in which Freud

adopted an unmistakably didactic attitude and engaged in active controversy with his analysand. Indeed, Freud is represented as breaking almost every rule of analytical technique; and, as regards his interpretations of the analysand's dreams and associations (so far as these are recorded), it is difficult to believe that the founder of psycho-analysis could be so obtuse. In general, one cannot help feeling that, however factual the account may be, its publication is in doubtful taste, even if it does not involve a breach of that discretion which the need for mutual confidence imposes on both parties to an analysis.

W. RONALD D. FAIRBAIRN

Eros and Civilization: a Philosophical Inquiry into Freud. By HERBERT MARCUSE. London: Routledge and Kegan Paul. 1956. Pp. xii+277. 25*s*.

Freud was led by the technical utility of his concepts of instinct and repression to suggest that civilization is possible only when the former is deprived of direct satisfaction by the latter. This forced him to conclude that his technique, which he conceived as the reduction of repression, could never be completely successful, even in theory, since the patient must continue to be a member of society and so must continue either to repress the more antisocial of his impulses or endure the tension of conscious self-denial. This pessimistic and somewhat *a priori* reasoning was attacked by Fromm, who was both a closer student of social history and an optimistic socialist. He questioned several of Freud's fundamental assumptions and suggested, for example, that aggression was not innate but implanted by the frustrations of a particular kind of civilization. The technique of the 'cultural' school of psychoanalysts in the United States stems from his ideas.

Prof. Marcuse dismisses them as 'neo-Freudian revisionists', but devotes an epilogue to arguing that while their theories may be therapeutically useful they are trying to dodge the crucial question: 'Must civilization entail the renunciation of instinctual satisfaction?'

He himself might be classified as a palaeo-Freudian Hegelian. He thinks that Freud's assumption of innate and immutable instincts was right, but that his conclusion was unnecessarily pessimistic. He argues, in the first place, that the instincts, and particularly *eros*, the pleasure-seeking group, have a natural tendency to sublimate themselves, and do not merely do so, as Freud thought, under the compulsion of civilization. His second argument is that Freud confused two kinds of repression. Repression proper is necessary if man is to live at all, let alone in society. What is imposed by social institutions is 'surplus repression' (although, as it is described by Prof. Marcuse, this sounds more like conscious or habitual obedience to restrictions).

He goes on to point out that both inside and outside society men have had to forgo instinctual satisfaction out of the sheer necessity of performing unpleasurable work; since he regards this as a special instance of the reality-principle he calls it the 'performance-principle'. But when civilization attains such a high degree of productivity that this 'alienated labour' is reduced to negligible proportions, this state of affairs will combine with the naturally sublimated *eros* to produce the 'freedom of a non-repressive civilization'.

If I have over-simplified a solution, particularly of a crucial problem, my excuse must be that Freud and Hegel make a confused duet, without the accompaniment of any concrete illustrations. It is true that the polarities to which Freud tended (ego and id, eros and thanatos, reality- and pleasure-principles) lend themselves to dialectic, and that Prof. Marcuse has a great deal of insight into both. But Freud's sociological assumption was only the speculative by-product of a therapeutic technique: Fromm was the sociologist, and it is startling to find his theories praised for their therapeutic value but dismissed in their proper field in favour of Freud's.

NIGEL WALKER

Psychotic and Neurotic Illnesses in Twins. By ELIOT SLATER, with the assistance of JAMES SHIELDS: Statistical Appendix by JOAN MAY. Medical Research Council Special Report Series, No. 278. London: Her Majesty's Stationery Office. 1953. Pp. vi+385. 21*s*. $4.75.

Dr Eliot Slater is Director of the Genetics Unit of the Institute of Psychiatry, University of London. His investigation was in the first place concerned with the standing population of 20,640 patients in ten hospitals for adult psychotic patients of the London County Council Mental Health Services. But, with two additional hospital sources of twins, 297 twin pairs were finally available for statistical analysis.

In the case of same-sexed twins, the anthropometric examination included measurement of height, of the hair colour (against a standardized range of human hair samples), eye colour (against a standardized scale of glass eyes), four skull measurements and the taking of finger-prints. The use of finger-prints here marks a definite advance in research technique. Dr Slater has worked out 'a method of reducing the total description of finger-prints to purely quantitative characters'.

The 297 twin pairs fall into four main diagnostic groups: (I) 158 pairs of schizophrenics; (II) 38 pairs in the Affective Disorders Group, 23 of whom were manic depressives or primary endogenous depressives; (III) 49 epileptics and organic psychoses; and (IV) 52 pairs of psychogenic behaviour disorders in children, psychopathic reactions occurring in mental defectives, besides other neurotic and psychopathic states. Sex, ovularity and clinical diagnosis of all twin pairs are tabulated. The four groups are compared for (*a*) size of family, (*b*) abnormality in family, (*c*) personality resemblance between uniovular twins, (*d*) twins separated in childhood and later life.

Two of the conclusions in (*b*) may be noted: '...general constitution and non-specific genetical factors are highly important in determining the manifestation of schizophrenia or of psychopathy or neurosis, but less so the manifestations of an affective or organic illness. The environmental hypothesis... cannot be excluded, but it is not plausibly reconciled with the difference between the schizophrenic and affective groups.' Secondly, there is suggested a relationship 'between bodily constitution and powers of resistance to schizophrenia'.

(*c*) Personality resemblance between uniovular twins—15 male and 39 female pairs—is discussed in some detail. 'In the absence of any pathological change, [they] develop closely similar personalities'.

(*d*) Only three pairs of twins were separated from early childhood until after they had left school. In only two cases, one binovular and one uniovular, 'is there anything like clear evidence that the different experience of the twins... contributed anything significant to their differing psychiatric fates'.

Factors predisposing to or associated with mental illness are tabulated and discussed. Unfortunately, as Dr Slater says, information is often scanty and not all twin pairs can be used. Psychologists of the extreme environmental outlook will need to ponder the finding that 'there is a marked tendency for the more severely affected twin to have been the more neurotic child, but this tendency, however, is shown only by the binovular twins....This suggests that the difference is genetically determined and that the constitutional make-up which predisposes to mental illness in the adult helps to cause neurotic symptoms in childhood'. As for neurotic traits in adult life, 'we can suggest first that the make-up of the personality affects the liability to psychotic as well as neurotic illness, and that environmental as well as genetical factors play a part in its causation'.

In Part I of the Report, Slater discusses in 22 pages twin investigation in psychiatry—its history, rationale and validity. He critically summarizes previous work, much of it in German, on unselected series of psychiatric twins.

Part III is the core of this investigation: the clinical investigation of the data on the four psychiatric groups. For each group in turn, duration, type of onset, precipitation, clinical symptoms and treatment are discussed.

Abnormal personalities found among first-degree relatives of the propositi in the four main groups were classified. Abnormal personalities in the schizophrenic families (75%) were much more markedly abnormal than those found in the affective families (54%). This may be an artifact. A colleague of the reviewer has recently found that different types of psychiatric patients differ in the extent to which they report psychiatric illness in the family history.

Slater writes in the epitome: '...genetical causes provide a potentiality for schizophrenia, perhaps an essential one, though environmental factors play a substantial role, which may be decisive in the individual case. Those environmental factors which affect personality and constitution appear to be the most important. Heredity is also an important factor in the causation of affective and organic psychoses.' 'There is a striking contrast between the psychoses on the one hand and the psychopathic and neurotic states on the other. In the latter, uniovular pairs were less frequently concordant, and some of the binovular pairs developed very similar troubles despite big differences in intelligence and personality.'

This research deserves the widest circulation. No one should graduate in psychology without some familiarity with its findings. It will help to balance the excessive emphasis on purely psychogenic causation current in many text-books and suggest a more reasoned expectation of 'cure' in psychiatric illness.

The critic may question the categories, the diagnoses and their reliability, and eventually the whole framework of clinical psychiatry as providing concepts suitable for translation into genes. (See Gardner Murphy, 'A research programme for qualitative Eugenics', *Eugen. Quart.* **1**, 209–12, 1954.) In the tradition of the best clinical psychiatry Dr Slater presents full case histories of all pairs of uniovular twins, of all pairs of doubtful ovularity and of concordant binovular pairs, and summaries of all other

cases. He is careful to point out where data are insufficient and to give due weight to alternative interpretations. Combined with this is a sensible use of statistics. Wherever possible data are tested for significance.

The psychologist will regret that more quantitative data derived from psychological tests were not available, but it is obvious that this would have involved another full-time research worker. In some case histories I.Q. (in places given as 'I.Q. 116 per cent') is listed, but other statements about intellectual level are presumably clinical judgements.

Dr Slater's research is assured of a permanent place in the world literature on psychiatric genetics.

J. C. KENNA

Training the Backward Child. By HERTA LOEWY. London: Staples. 1955. Pp. 166. 12*s.* 6*d.*

In this book, written for parents and teachers of backward children, Miss Loewy draws upon her first-hand experience of living and working with backward children over a considerable number of years. This follows her previous book, *The Retarded Child*, carrying her ideas further and describing in some detail methods of teaching by means of games, stories, jingles, music and mime. Her claim that she can help the child classed as 'ineducable' is substantiated by the interesting case studies that she includes.

The style is simple and homely, which is rather irritating to the professional reader, but may commend itself to the layman; the sincerity and enthusiasm of the writer are evident.

Miss Loewy stresses the importance of working in close co-operation with the physician and of early physical treatment for the backward child. She writes realistically and sympathetically of the attitudes of parents to their backward children. 'The effect on parenthood of having a backward child is exactly what you choose to make it—make it something constructive that will help the child' is her plea. She has some frank but wise things to say about grandparents.

The chapter on religious education is too brief, and one looks in vain for a clear statement concerning the spiritual needs of the children under her care. However, the importance of simplicity and clarity in presenting religious concepts to the backward child so that 'his use of religion' does not become 'a hollow mockery' is rightly stressed. She includes a number of rhymes, playlets and stories which are devised for the enjoyment of the children, to increase their self-confidence, to improve their social adjustment and their general behaviour. It is questionable how greatly these may influence their moral development, which is one of her aims.

Diets found useful for combating acidity, constipation, undernourishment, obesity and digestive troubles are described at some length. The contention is made that 'a digestively happy child is much more likely to make mental progress'. Considerable emphasis also is laid on the value of exercise.

There are many practical points raised in this book, and much useful guidance given to both parents and teachers. The material is rather fragmentary and not arranged in a very satisfactory way, but the careful reader, who is concerned with both the social and the educational problems of the backward child, will find much that is interesting.

A. H. BOWLEY

Emotional Problems of Early Childhood. Edited by GERALD CAPLAN. London: Tavistock Publications. 1955. Pp. xiv+544. 42*s.*

This book consists of twenty-four papers selected from those presented to the Institute of Child Psychiatry in Toronto in August 1954. It is divided into three main sections: preventive aspects of child psychiatry; the relation of physical and emotional factors and problems of hospitalization; psychosis in early childhood. Each section contains case histories, research reports and a general commentary by such eminent writers as Drs Gerald Caplan, Frederick Allen and Marian Putnam. These commentaries are probably the most valuable part of the work.

The main emphasis of the book is, rightly, on prevention, and on the need to detect and modify conditions in the family and in the wider social environment that are likely to lead to pathology in the child. Several papers deal with the important problem of devising objective methods of observing and classifying behaviour (including verbal behaviour). It is too early, yet, to judge the value of this research. Stone describes his findings as 'a few tentative impressions and hypotheses and a plethora of questions'. One feels that publication in these cases is perhaps premature. A number of faults appear in the research reports, which even the great difficulty of the task does not justify. These include ambiguous statement

of results, wide generalizations drawn from inadequate numbers, and, in one case, a control group of twenty children, ranging in age from 3 to 15 years. In marked contrast to these are the excellent reports of Beata Rank on children showing 'atypical' development, treated at the Putnam Center in Boston during the past ten years, and of Lauretta Bender on twenty years of work with schizophrenic children in Bellevue Hospital, New York.

The case studies, though interesting, present no new developments in either diagnosis or treatment. In all cases, the outcome and, in some, the etiology is in doubt. It is unfortunate that the one chosen to illustrate Mahler's interesting concept of symbiosis in childhood schizophrenia should be that of a child where neither the psychosis nor the symbiotic relationship are satisfactorily demonstrated.

The book is a valuable record of the progress in Child Psychiatry up to 1954, but it is not, as claimed on the dust cover, 'an authoritative statement on the diagnosis, treatment and prevention of emotional disturbance'.

JOYCE COOKSON

La Perception. Symposium de l'Association de Psychologie scientifique de Langue française. Paris: Presses Universitaires de France. 1955. Pp. 128. 500 fr.

The *Association de Psychologie scientifique* devoted its second annual meeting held in Louvain in 1953 to perception. This symposium contains papers read at the meeting and the discussion which followed some of them. The book falls into two parts. Part I has for its theme 'the influence of experience on the organization of sensory data in perception'. It includes papers by Piéron, Piaget and Michotte, and the subsequent exchange of views. Part II is a more heterogeneous collection of four papers: 'Perception and information' by Ombredane, 'Perception and personality' by Nuttin, 'First responses in the infant and their stimuli' by Fauville, and a discussion by Koch of 'a possible link between physiology and behaviour in the migratory fish and birds'.

The theme of the three *rapports* which form the body of the first part of the book is, of course, a lively controversial issue at present. A publication which contains concisely stated views of Piéron, Piaget and Michotte on the subject cannot fail to be of great interest. Piéron breaks his lance in defence of a far-reaching empiricism. His paper offers a review of the evidence supporting this position: experiments on sensory deprivation and on perceptual constancies, and the work of Ivo Kohler. Theoretically, he achieves a not unfamiliar blend of Hebb's ideas and of the Princeton transactionalists' treatment of the role of action in the development of perceptual organization.

Michotte represents the 'nativist' extreme of the range. His paper is a plea for a careful 'structural analysis' of the external determinants of perception. This, as exemplified by Michotte's earlier work on the perception of mechanical causality and his more recent studies on 'substantial permanency', would yield possibilities of predicting the phenomenal percept. A parsimonious explanation of some fundamental aspects of perceptual organization could thus be achieved without recourse to past experience.

Piaget's paper provides a link between Piéron's and Michotte's contributions. He applies to problems of perception his concept of equilibrium which has, according to him, a higher degree of generality than either 'innateness' or 'experience'. One may regret that a more specific account of the operation of laws of equilibrium in perception is not given. In the course of the discussion Piaget mentions that someone has asked him 'what he meant by equilibrium'. His reply is 'reversibility in general'. However, in the context of this paper the notion of equilibrium stands a little isolated, and sometimes may even appear to have the status of a final cause. A number of fascinating experiments are described in the paper, some of them very recent, drawn from Piaget's work on perceptual development in children. They fully support a distinction drawn by Piaget between perceptual potentialities of an organism, which may be innately determined, and the contents of perceptual experience which are based in very large measure on slowly acquired and progressively more complex co-ordinations.

This section of the book is worth reading more than once. In addition to providing a concise and thoughtful exposition of some of the most interesting aspects of contemporary psychology, it also reveals some of the cracks in the foundations. There seems to be a curious lack of communication between the various contributors. Piéron and Michotte carefully select the evidence which they use to support their views; each of them seems to avoid, almost as carefully, a thorough examination of the evidence brought forward by the other. As a result an almost identical argument woven into both papers is used to support opposing points of view. It is based on experiments in which the percept is so compellingly determined by some aspects of the 'field' that it can hardly be modified, even when the subjects know that the physical arrangements of the experiment are not what they appear to be. For Michotte, who refers to his work on the perception of causality, this demonstrates the importance of 'pre-established structures',

independent of experience and knowledge. Piéron, who describes the well-known experiments showing the effects of 'object-qualities', draws a generalized conclusion in favour of past experience.

The role of motivation in perception is discussed in the papers by Ombredane and Nuttin. Ombredane's contribution is an eclectic and not very explicit attempt to bring together some concepts from the ex-'New Look' and information theory. Nuttin's paper deals with the perception of success and failure. It is a very useful summary of the experiments and theoretical discussions included in his recently published large volume on *Tâche, réussite, et échec. Théorie de la conduite humaine.*

The remaining two papers offer interesting accounts of experimental work, but they are hardly related to the central theme of the symposium.

H. TAJFEL

The Practice of Modern Perfumery. By PAUL JELLINEK (translated from the German by A. J. KRAJKEMAN). London: Leonard Hill. 1954. Pp. x+219. 25*s.*

Basic Odor Research Correlation. By R. W. MINER (ed.). *Annals of the New York Academy of Sciences*, volume 58, art. 2, 1954. Pp. 13–260. 24*s.* 6*d.*

These two books are important contributions to a comparatively neglected area of study. *The Practice of Modern Perfumery* contains the sort of 'inside information' which is rarely made readily available. It is a book by a practising expert. The first and last of its four (roughly equal) sections are rich in information and interesting hypotheses relevant to psychology. The first section includes instruction on how to devise and to carry out a 'suitability test' for those who might wish to become perfumers. The technique of 'olfactory analysis', which makes use of the well-known phenomena of olfactory fatigue, is described in sufficient detail for those interested to attempt such an analysis for themselves. However, the roles of training and experience are also made explicitly clear. The last section of the book is broader in scope and may be of wider interest to psychologists than the first. It is entitled, 'Perfumery, cosmetics and psychology'. The role of natural (clean) body odour as a source of sexual stimulation is discussed at some length, with supporting evidence. The author suggests that certain odours of an animal type produce pleasurable associations providing the 'intensity' of the odour is too low for it to be identified as having direct animal associations. He stresses the importance of visual associations. For instance, he quotes evidence to indicate that certain floral perfumes are unpleasant except when they are *directly* associated with the relevant flowers. He describes a four-fold system for classifying odours contrasting 'Erogenic' (sex-stimulating) with 'Anti-erogenic' (refreshing and disinfectant), and 'Narcotic' (intoxicating) with 'Stimulating' (activating, exciting). Later, the relation between perfume and personality is examined in this framework.

These are just a few of the many items of interest to psychologists. The experimental psychologist will find technical information of immediate value. The student of personality and the clinical psychologist will also find much to interest him. Observations on perfume and fashion would interest the social psychologist. In short, it seems reasonable to claim that, in the first and last sections of this book, there is something for everyone concerned with contemporary psychology.

The second book is a collection of papers by some forty specialists brought together under the title of *Basic Odor Research Correlation.* Bernice M. Wenzell writes on 'Problems of odor research from the standpoint of the psychologist'. R. W. Moncrieff (the only British contributor) writes on 'The odorants'. Jean F. Mateson writes on 'The olfactory area and the olfactory receptor process'; M. M. Mozell and Carl Pfaffman on 'The afferent neural processes in odor perception'; and K. H. Pibram and L. Kruger on 'The functions of the "Olfactory brain"'. R. M. Hainer *et al.* supply a stimulating paper entitled 'An information theory of olfaction'. This selection is essentially a personal one and in no way implies that the other contributions are less interesting or less important. In a speculative introduction A. R. Behnke outlines what the state of affairs in odour research might be in 1983, thirty years after the meeting organized by the American Society of Heating and Ventilating Engineers, at which these varied papers were given. At first sight this somewhat facetious approach might seem disturbing in a serious scientific publication. However, it reflects precisely the general impression which emerges from reading this collection of papers. New techniques are being developed and new knowledge, which seems to be arising as a result of those techniques, suggests that much that is now accepted and written into the *few* pages devoted to the sense of smell in most familiar text-books is likely to be drastically revised and greatly extended in the next two decades. This, like Jellinek's book, can be recommended as an essential addition to the psychological reference library. Those who have more of a specialist interest will no doubt want their own copies.

ROLAND HARPER

Manuel Pratique de Psychologie Expérimentale. By Paul Fraisse. Paris: Presses Universitaires de France. 1956. Pp. xi+312. 1.200 fr.

After a lucid, thirty-nine page discussion of the nature of psychological experimentation, this book presents sixty exercises for use in an introductory course in experimental psychology. For French psychologists it supplies a definite need since, according to Fraisse, no other such manual exists in their language. British psychologists who are concerned with organizing practical laboratory courses are more fortunate in having several fairly up-to-date guides at their disposal. For them, this manual contains little that is new, unless it be the inclusion of a small number of Michotte's demonstrations of the perception of causality. This book is a reminder that a really satisfactory set of laboratory demonstrations has not yet appeared. It seems to the reviewer that, in Britain alone, psychology departments could collaborate to provide a core of standard experiments for which no apology need be made to the student on behalf of this or that inadequacy and for which the accumulated results of many student subjects would be available. This might do more than anything else to engender in the novice a respect for and an understanding of experimental method.

I. M. L. HUNTER

The Evolution and Growth of Human Behavior. By N. L. Munn. Boston and New York: Houghton Mifflin Co. 1955. Pp. x+525. 35*s*.

Teachers of psychology have frequently to contend with the problem that members of their classes approach the subject with very different backgrounds and frequently with no background at all in the biological sciences of which genetics, and some orientation in regard to phylogenetic development, are especially helpful. Dr N. L. Munn's *Psychological Development* (Houghton Mifflin Co., 1938) was in this respect a particularly useful book which provided a commendable amount of information, a wealth of reference and some clear introductions to fundamental issues. One recalls, for example, the discussion of the James–Lange and the thalamic theories of emotion. Unfortunately, the book was in recent years unobtainable in this country. The present work is intended to replace it.

There are many points of similarity between the two books, since much of the material and illustration in the earlier work is still relevant. The new work maintains the high quality of printing, paper and binding, but the page is larger ($7 \times 9\frac{1}{2}$ in.), and the text is arranged in two columns. The sequence of topics is broadly the same as in the earlier work, but the treatment has been extended to include more recent experimental results. The seventeen chapters include those on 'The nature of genetic or developmental psychology', 'Unlearned behavior', 'Prenatal behavior', 'Basic factors in the behavioral development of children', 'Development of spatially coordinated behavior', 'Motor development', 'Language', 'The growth of intelligence', 'Emotional development', 'Development of social behavior' and 'Changes in personality from adolescence until senescence'. Naturally the wide range of reference has required some selection of the studies used, and the author's claim to have done this eclectically is justified. The exposition is consistently clear. The book could be read by the student very early in his university course. With its ample references and suggestions for further reading, together with extensive indices of subjects and of authors, it provides a clear and broad introduction to the vast literature associated with the concept of development. The impression does arise that the treatment is less intensive than in *Psychological Development*; but those who seek more are provided with ample and specific further references.

F. V. SMITH

Der Ausdrucksgehalt des menschlichen Ganges. By Gertraud Kietz. Leipzig: Johann Ambrosius Barth/Verlag. 1956. Pp. 174. DM. 10.20.

Manner of walking as an expressive movement has always attracted much attention on the Continent, and this monograph contains a good survey of the literature. It also attempts to add to our knowledge by means of what the author mistakenly calls 'a well-designed experiment'. The idea apparently was to compare certain personality traits of her subjects with their behaviour in a situation in which they had to move from one place to another along predetermined lines. The results of this 'experiment' are impossible to evaluate because of the small number of subjects, the subjectivity of the appraisal, the invalid nature of the measuring instruments and the failure to treat the results in any statistical way whatsoever. The author, although quite knowledgeable as far as the Continental literature is concerned,

does not give a single reference in English, and is apparently ignorant of the important work done in this field by such writers as Allport and Vernon. Had she been familiar with their twenty-years old *Studies in Expressive Movements*, her 'experiment' would not have been as inconclusive as it was.

H. J. EYSENCK

Gestalt Therapy: Excitement and Growth in the Human Personality. By FREDERICK S. PERLS, RALPH F. HEFFERLINE and PAUL GOODMAN. London: Pitman. 1952. Pp. xiii + 466. 45*s*.

This book attempts to lay the foundation for the 'full application of Gestaltism in psychotherapy . . . to develop a theory and method that would extend the limits and applicability of psychotherapy'. The reader is invited to do eighteen experiments with himself; these are arranged in groups, 'orienting the self, and manipulating the self'.

Accompanying the detailed instructions and discussion of these 'experiments' are the comments selected from 100 undergraduates who did these experiments as homework.

The second part of the book discusses 'basic neurotic dichotomies of theory', body-mind, self and external world, etc., and leads up to 'a theory of self and creative action'. There is a critique of psychoanalytic theories, and recognition of the influence of Korzybski, Reik and non-orthodox analysts.

The book is too long. The practising psychotherapist may, according to his school, derive some stimulus from it. The academic psychologist is more likely to want answers to more basic questions before taking time off for it. The book has no index.

J. C. KENNA

The Great Mother: An Analysis of the Archetype. By ERICH NEUMANN. Translated from the German by RALPH MANHEIM. London: Routledge and Kegan Paul. 1955. Pp. xliii + 380 + 185 plates. 50*s*.

There can be no doubt that Erich Neumann, German born, working in Tel Aviv, is one of the most creative and original analytical psychologists. *The Great Mother* is suitably dedicated 'To C. G. Jung, Friend and Master in his Eightieth Year', and the admirers of Neumann's earlier works, especially of the already classic *Origins and History of Consciousness* (1954), will not be disappointed by this latest creation. His style is lucid and elegant in its perfect translation, and Neumann knows how to write a psychological dissertation which at the same time is an aesthetic feast and a learned tome. The anthropologist, even if he may not always agree with the author's deductions, will find much of special value in the vast material presented, while the artist and art student may be deeply interested in Neumann's interpretation of so many works belonging to the most various ages and cultural spheres. The analytical psychologist, however, will be the one to profit most from this clear exposition of archetypal life in the psyche of man. 'What is an archetype?' ask the anti-Jungians somewhat disdainfully; I think that even the most stubborn resistance against the understanding of this conception of analytical psychology is bound to crumble in the face of this masterly explanation.

Apart from the elaboration of this Jungian principle the author adheres strictly to the task of investigating the particular archetype of the Great Mother. In doing this he works throughout with only a very few fundamental ideas regarding its nature which he explains at the outset. He faithfully carries out his research and resists the temptation of being drawn into the discussion of side-issues. Diagrams help to give the greatest possible clarity to the strategy of his working method; and, whatever one may think about diagrams in general, these are really helpful and further reveal Neumann's most original mind. Special praise is due to the way in which these 'schemata' can be unfolded and kept before the reader's eye while he studies the text relevant to them. The reproduction of the many illustrations is of the highest technical and artistic standard, and the publishers have, indeed, succeeded in producing a most valuable and beautiful book.

W. P. KRAEMER

Individuation: A Study of the Depth-psychology of Jung. By JOSEPH GOLDBRUNNER. London: Hollis and Carter. 1955. Pp. viii + 204. 21*s*.

This work has been translated from the German by Stanley Godman, and is divided into two parts. The first part deals with Jung's approach to psychology and the problems of personality, neurosis, the unconscious, the treatment of psychological disorders, the archetypes and religious experience. This occupies

156 pages and, as an exposition of Jung's ideas, it is about as clear as would be possible. The second part is much shorter, and is devoted to an examination of the bearings of Jungian psychology on religion, anthropology, ethics, education and the cure of souls. Many of the points set forth in this part of the book will be of considerable interest to students of Jung.

R. W. PICKFORD

Studien zur analytischen Psychologie. Two vols. Edited by the C. G. Jung Institut, Zurich. Zurich: Rascher Verlag. Vol. 1, pp. xi+396. Vol. 2, pp. ix+397. Swiss francs 22.90 each vol.

When Prof. Jung celebrated his eightieth birthday last year, the Zurich Institute which bears his name presented him with these two volumes of collected articles. Their various authors have all, at one time or other, lectured at the Institute which, in the seven years of its existence, has faithfully served the aim of its founders to act as a centre of teaching and research. In the introduction by the Board of Governors, it says of the authors: 'Interest in analytical psychology and gratitude for everything that they have received from Jung is common to all of them.' In spite of the great difference in background of the thirty-two contributors, this cross-section of the work done at the Institute tells, in fact, a vivid tale of the harmonious growth of a new attitude, based on the *Weltanschauung* which is connected with Jung's name.

The first volume contains 'contributions to theory and practice'. The second is devoted to the history of civilization.

While it is not possible to give an individual appreciation of the articles here, I should like to mention that Mrs Jung, whose recent death has saddened us, is one of the authors, and that her contribution on the Anima is a most beautiful and sensitive fruit of her wisdom and knowledge. She, of course, like most of the authors of the 'Studies', writes in German, but there are also a few short French essays and a considerable number written in English. As the latter may be of particular interest to British psychologists, I mention some of their writers by name: E. A. Bennet, whose casuistical sketch 'The Double' is particularly interesting, is one of Jung's oldest friends. Michael Fordham and Gerhard Adler are both leading figures in the London Society of Analytical Psychology. Father Victor White of Oxford and Frances G. Wickes of New York have for many years been most creative interpreters of Jung's work. But there is no space to mention many other equally distinguished names. There is E. Neumann of Tel Aviv, for instance, whose general contribution to analytical psychology is quite outstanding and whose article is of the very highest quality, or K. Binswanger, B. Hannah, J. Jacobi, M. L. v. Franz, who all belong to the circle of Jung's closest collaborators.

The books make fascinating reading. Many of the papers will be of equal interest to the general reader, who has an interest in 'the new attitude', and to the specialized psychological research worker and the analytical psychotherapist. Jung's movement is so fully alive and of such dynamic power that a study of this work is certain to stimulate the 'analysed' and 'unanalysed' alike.

W. P. KRAEMER

Narcissus: A Psychological Study of Self Love. By GRACE STUART. London: Allen and Unwin. 1956. Pp. 166. 12*s.* 6*d.*

This is a short and very readable, but rather diffuse, book in which psychoanalytic ideas combining strictly Freudian concepts with those of Melanie Klein, Fairbairn and others are set forth and applied. The book has literary merit and is illuminated with many apt quotations from poetry, plays and other works as well as showing psychoanalytic erudition. The author is to be commended for a clear grasp of the problems of early infancy from the point of view of the infant, and narcissism is understood and presented with this insight. The earliest infantile relation with the object is one of identification, and narcissism is not only a phallic level problem, but very much more of pre-genital and indeed of early oral origin. If the book helps some of the public interested in psychoanalysis to grasp more clearly these points, which are so painfully obscure to many people, then it will do a signal service.

R. W. PICKFORD

The Psychology of Economics. By WALTER A. WEISSKOPF. London: Routledge and Kegan Paul. 1955. Pp. viii+266. 25*s.*

This book is concerned with an analysis of factors which influence the choice and retention of economic concepts. The questions of bias in economics, and of the place and influence of value judgements, have

occupied social scientists for many years. Weisskopf, in stressing the existence of unconscious bias, approaches the problem from a psychoanalytic angle, and is probably one of the first writers to claim that bias in the selection of economic concepts fulfils an important psychological function.

Weisskopf sets out 'to explain the function and meaning of some economic ideas with the help of the psycho-cultural method'. He holds that economic thought must be viewed in the context of psychological conflicts which are caused by cultural and socio-economic conditions. Thought-constructs must be considered as a reaction to anxiety-creating conflicts and a way of dealing with them. In uncovering the unconscious function of thought, Weisskopf goes beyond psychoanalysis by postulating that 'basic human conflicts are not only of a biopsychological but also of a social, intellectual, and spiritual nature'. Individuals share many conflicts with their contemporaries, and systems of socio-economic thought provide solutions of supra-individual social importance.

In a series of well-documented case-studies, Weisskopf examines the value theories of Adam Smith, Ricardo and Marshall in the light of conflicts in the 'value-complexes' of their times. In the most provocative part of the book, he discusses male and female symbolism in the thought of Ricardo, Malthus, Engels and Marx. Many of the arguments about symbolism appear far-fetched; the author is more convincing where he deals with the influence of factors other than biopsychological ones, and it would be a grave mistake to reject all his arguments, because some are unconvincing. The explanations of contradictions in Marshall, for instance, are very striking.

This book raises important issues which should be of interest not only to economists but to all social scientists. Parts I and VI are probably of greatest interest to non-economists.

HILDE BEHREND

Guide du Psychotechnicien. By C. G. Fontaine. Paris: Dunod. 1956. Pp. 119. No price given.

The interest of this little book for British psychologists does not lie in the chapters which aim at advising the intending psychologist or informing the lay public. It lies in the veritable mine of information about the status, the formation, the prospects and the worries of psychologists in France. A university degree in psychology is a recent development (it was instituted at the Sorbonne in 1948), and prospects for graduates are not too bright. Training in some applied fields (e.g. vocational guidance and selection) has reached high standards; in others, such as clinical and educational psychology, it lags behind considerably. Conditions of employment are often ill-defined.

An interesting account of public reactions to the practice of psychology is given. It seems that some employers stand in urgent need of the guidance provided by this book; an industrialist was recently seeking a psychologist 'who would also be capable of administering first aid and dealing with the wages of the personnel'. In the trade unions, the tradition of resistance to Taylorism has largely carried over to attitudes towards industrial psychology, but there are some hopeful signs of change. The medical profession insists on adequate scientific standards. These include a firm dismissal from psychological assessments of any tests of 'psychoanalytic nature'.

The author feels that legal and ethical codification would help a great deal. There is an appendix containing detailed suggestions for an ethical code. This should be found topical by many.

H. TAJFEL

Leadership Qualities: a Theoretical Inquiry and an Experimental Study on Foremen. By Kullervo Rainio. Helsinki: The Finnish Academy of Science and Letters. 1955. Pp. 211. 750 marks.

Since its foundation in 1947, the Institute of Industrial Administration of Finland has been concerned with training in leadership skills in industry and, inevitably, with selection. This book is an interim report of research in the selection of foremen in which 700 industrial supervisors were tested and twenty-eight different tests explored, ranging from intelligence, through group discussion to Rorschach.

To find a way among the many personality traits that have variously been claimed to be needed by leaders, and bearing in mind the argument that the group structure rather than individual personality is critical, the author starts by making a theoretical analysis of 'leadership' as such. This he does in Lewin's topological terms, and distinguishes four conditions of interrelationship between the forces in the social field necessary for 'leadership' to occur. Stepping across now, a little anxiously, into non-topological terms he hypothesizes four personality characteristics, or 'leadership qualities', that will 'on the average

most often make possible fulfilment' of these relational conditions. In the light of his analysis he labels these non-suggestibility, energy, suggestivity of personality or authority, and maturity or ability to maintain direction in a conflict situation; intelligence appears to be of minor importance.

Tests were then given which offered assessments of these traits, although tests used in other countries for the selection of supervisors were also explored. Four criteria of leadership were used: a rating by superior for 175 of the men, a rating by Institute instructors for 22, a 'sociometric' rating by fellow-students for 117, and a criterion based on the efficiency of each foreman's shop for 79. There was a slight overlap; three different criteria were obtained for 22 of the men, and two for something under 38 others.

Five factor analyses were made, and the results combined by Ahmavaara's transformation analysis. Four factors were discerned and identified as general intelligence, non-suggestibility, stability and a problematical one called activeness. The first three had significant value in assessing leadership qualities, intelligence having least value, so confirming three of the hypotheses. No confirmation was found of the 'authority' hypothesis, which is taken to suggest that 'authority' is an empirical consequence of the high level of other leadership factors. The hypothesis about 'energy' remained doubtful.

The loadings suggest a test battery of intelligence and arithmetic tests for factor 1, body sway and File's 'How Supervise?' for factor 2, and leaderless group discussion, Maudsley Medical Questionnaire as dichotomous, and an intriguing distraction test for factor 3. These give a theoretical multiple correlation of $+0{\cdot}59$, with rating by superior, and in a try-out on 26 subjects a correlation of $+0{\cdot}42 \pm 0{\cdot}17$ was obtained.

The results of the study could be important. The group task test again comes up strongly, correlating $+0{\cdot}38$ with rating by superior, and again it is shown that evaluations by different observers correlated as highly as $+0{\cdot}7$. Other results are less convenient. The battery is a long one—two hours of group tests, two hours of tests in small groups, and twenty minutes of individual tests. Yet in a N.I.I.P. study a single intelligence test has given as high a correlation with supervisory performance as does this whole battery. More important, Rainio's correlation between intelligence and rating by superior was as low as $+0{\cdot}08$, although other studies have found correlations ranging up to $+0{\cdot}6$.

This makes us look again at the present study, when it appears that the criterion of rating by superior is of doubtful value. This was obtained by questionnaire (apparently to be returned by the foreman himself), and cross-checking suggests that the 175 completed were out of some 300 sent out; moreover, the ratings were by different raters, in different firms. In the end, most of the foremen reported on were rated as in the average to above average area, 4 points of the 9-point scale covering 86% of them.

Another point is that different groups of subjects did largely different tests, although it is claimed that since the groups consisted of foremen gathered at random there is no significant difference between them. The sort of combining of results involved appears when one examines closely which groups did which tests. Looking at the tests suggested for the battery it turns out that of the 700 men tested only 63 actually did all of them. For 10 of these men there was no criterion assessment at all, for the other 53 there was the sociometric rating, and for fewer than 16 of this 53 there was also the rating by superior. Looking at the intelligence test results in this way, correlations are quoted of $+0{\cdot}31$ with group task ($N = 442$), $+0{\cdot}25$ with sociometric rating ($N = 117$), and $+0{\cdot}08$ with rating by superior ($N = 175$); but in fact only 38 subjects could have been common to all these samples, or even to the last two.

Clearly a tremendous amount of exploratory work has been done and a review cannot do justice to it all. Yet doubts about the criterion and about the combining of results make one feel that two or three much smaller scale studies, each complete in itself and each in the context of a single firm, might have carried more conviction even if they involved small samples. Moreover, the author himself argues that the conditions of leadership are relational, citing, for instance, that the intelligence a leader needs is related to the intelligence of the people he leads; yet his approach is one that is bound to swamp out this kind of point. None the less, he has made a worth-while attempt to put his research into a wider theoretical framework, although the combination of topological psychology and factorial analysis, all in translation from the Finnish, make solid fare for the reader.

R. M. MCKENZIE

The Presentation of Self in Everyday Life. By Erving Goffman. Edinburgh: University Social Sciences Research Centre. 1956. Pp. 162. 10*s*.

This book sets out a new way of looking at social interaction, using the analogy of the theatre. The author shows how a great deal of social behaviour can be regarded as an attempt to put on a show for an 'audience', in order to create in them certain perceptions of the 'performers'. He distinguishes between

'front regions', where the performance takes place, and 'back regions', where the team of performers can relax and crack jokes about the audience. This analogy is followed up with many ramifications, and illustrated from the sociology of the professions and commerce, anthropological studies, books of etiquette and literature. This book breaks all the rules—the evidence quoted is largely anecdotal, the terminology introduced is often vague and cumbersome, and testable hypotheses are not put forward, though they could be dug out. On the other hand, it is quite fascinating and, it may be said, most illuminating. This is partly because many of the examples are quite hilarious, partly because the whole point of view really does give increased insight into social situations.

It may be objected that this is a *tour de force*, and that the author has exaggerated the extent to which behaviour is motivated by the need to create certain impressions—it would be more usual to postulate a continuum extending from pure acting, through lesser forms of role-playing, to involuntary twitches at the other end. It may also be pointed out that many other needs are operating in social behaviour besides that of manufacturing perceptions about oneself. Nevertheless, the impact of the examples collected together in this book is to give a convincing demonstration that a great deal of social behaviour can be analysed in this way, and that this process is probably more widespread than has usually been thought.

MICHAEL ARGYLE

Family: Socialization and Interaction Process. By TALCOTT PARSONS and ROBERT F. BALES. London: Routledge and Kegan Paul. 1956. Pp. xvii+422. 30*s*.

This book consists of variations on the theme of the family treated as a small group. In an introductory chapter, Parsons looks at the contemporary American family and argues convincingly that the high divorce rate does not necessarily indicate a progressive disorganization of the family as an institution; he believes it is the result of strains involved in changes in the roles of the marriage partners, changes that will in the long run actually strengthen the marriage ties.

In the next three chapters, the process of socialization is examined in a manner somewhat unfamiliar to psychologists, though readers of Parsons's previous works will not be entirely unprepared. As far as the reviewer understands, Parsons postulates a basic parallel between what he calls a 'personality system' and a 'social system'; it follows that the method of analysis used for the latter will also be appropriate for the study of the 'personality system'. Hence the development of the child is viewed as the progressively more complex internalization of 'patterned action systems', i.e. roles in relation to the other members of the nuclear family. Roles within the family are classified into various types, and Parsons holds that the child's learning proceeds by 'binary fission', analogous to cell division in organisms; thereby the original simple 'mother-child system' becomes differentiated to include the father and siblings, first in relation to their intra-familial roles only, and later in relation to their roles in the wider social structure. The following illustration will show that this is a gross over-simplification of Parsons's position as formulated in his characteristic style:

'However, we may suggest that the step from four to eight consists in a bifurcation of the instrumental-expressive category by a cross-cutting differentiation of universalistic and particularistic aspects. The bifurcation on the instrumental side produces a distinction which has sometimes been referred to as the difference between technical and "executive" roles.

'The parallel differentiation on the expressive side is that between integrative functions which are primarily particularistic, and pattern-maintenance-tension-management functions which are primarily universalistic in focus.'

The main lesson for psychologists which he drives home repeatedly is that biological needs or drives '*cannot* serve as the primary basis for the organization of personality as a system'. They are limiting, not determining factors; organization is brought about by social interaction and implies control over organic needs. One of Parsons's few critiques of psychoanalytic theory concerns its model of 'a "linear" branching system of motivational strands which then operate independently of one another'. Otherwise his stages of role-differentiation are closely parallel to the Freudian stages of psycho-sexual development. Perhaps it would be legitimate to say that he has in fact translated the somewhat loose psychoanalytic account of child development into the rigid framework of a sociological role-theory. In the process all the life has inevitably been drained out of it; the question is of course whether any gain in scientific insight has resulted.

Before discussing this, some mention must be made of two other parts of this work. There is, first, a chapter by Bales and Slater on 'Role differentiation in small decision making groups'. It describes a

series of experiments with college students which seem to have a very slender bearing on the central topic of the book. The main outcome of these investigations was to demonstrate that role differentiation did occur in these groups. The hypothesis emerges that 'the tendency toward differentiation must arise out of factors associated with social interaction over a very broad range of situations'.

There is also a carefully executed and cautiously presented comparative study by Morris Zelditch. It deals with role differentiation within the nuclear family in a variety of different cultures. This lends some support to Parsons's claim that the division of intrafamiliar roles he describes represents a widespread, if not universal pattern. Zelditch suggests that variations in parental roles may be a function of the relationship of the nuclear family to the wider descent groups.

What is liable to strike the psychologist most, and probably unfavourably, in this book is Parsons's almost complete lack of reference to concrete pieces of behaviour. We are confronted with a formal hypothetical system at a high level of abstraction. This in itself is of course not a criticism; on the contrary, the social sciences are often accused of unbridled empiricism, the random collection of data unordered by any coherent theoretical framework. The crucial question is whether Parsons does in fact provide such a framework. For instance, is the Freudian theory of psycho-sexual stages more amenable to verification in its Parsonian form? Or can any other testable consequences be derived from his system? The reviewer does not feel confident of having a sufficient grasp of Parsons's thought to venture a definite answer. His own is perhaps a little disappointing:

'If our view is correct all the main pathological syndromes should be found in all human populations. Their relative incidence, however, would be expected to vary substantially... This should prove to be a testable claim.'

Although one may suspect that the attempt at a synthesis in the grand manner is premature, the work has much to offer with regard to specific problems. An example is Parsons and Olds's treatment of such topics as internalization or frustration, which is original and thought-provoking. A good deal can be learnt by looking at familiar problems from an unfamiliar angle—various aspects become apparent that one had previously missed. This will be the reward for psychologists with sufficient perseverance to struggle through this book.

One last complaint: there seem to be more than the usual quota of printing errors, and page 352 is blank.

G. JAHODA

Democracy and Dictatorship: Their Psychology and Patterns of Life. By ZEVEDEI BARBU. London: Routledge and Kegan Paul Ltd. 1956. Pp. viii+275. 28*s*.

Dr Barbu's book is the latest attempt to describe and analyse the characteristic habits of response in democratic and totalitarian countries. He has avoided most of the errors of his predecessors, including that of over-stressing patterns of child care, and has endeavoured to do justice to economic, sociological and historical factors. Indeed, the author's background as a European academic and diplomatist who has had first-hand experience of both German and Russian forms of totalitarianism gives weight to his psycho-social analysis and provides it with a balance often lacking in other studies in this field.

The book is in three parts, treating respectively the psychology of Democracy, Nazism and Communism. In each case a few characteristic features are singled out for detailed study. Thus, for instance, the processes of social and cultural flexibility and the rationalization and minimization of power are taken as characteristic of all democratic societies, whilst processes of social and cultural rigidity, and the emotionalization (Nazi) or super-rationalization (Communist) of life together with the concentration of power are characteristic of modern totalitarian societies. Perhaps two of the most interesting sections of the book are the discussions of the replacement of religion by dogma and the pattern of individuation in communist societies, the arguments here being both subtle and convincing.

Two criticisms may be made. First, Dr Barbu's attempt to discuss the ideal-type personality structure of the communist with reference to the personalities of Marx, Lenin and Stalin, fails to strengthen an argument which does not depend on it. Secondly, his very close identification of the democratic pattern of life with Britain is generous rather than realistic; it ignores various authoritarian and rigid elements easily discernible in schools and industry.

In general, this book is most readable and one of the best in the literature of its kind.

DUNCAN MITCHELL

To Define True Madness. By HENRY YELLOWLEES. London: Pelican. 1955. Pp. 172. 2*s.* 6*d.*

This book can be read with enjoyment and profit by anyone who is interested in medical psychology. Much of the raw material will be familiar, but it is presented here in a lively and original manner. Dr Yellowlees, well versed in the Bible, Shakespeare and English literature generally, is an excellent writer, always lucid and forceful, and often witty. An expert at quoting aptly, he often writes quotable prose himself: 'You should talk to the insane, generally speaking, as if you were talking to the sane. You can never go far wrong if you can do that, provided that your talk to the sane is worth hearing.'

His discussion of insanity as a defence in law is important, for he has more practical experience of this problem than most of his colleagues. 'It is impossible to give a clear or accurate clinical picture of mental disorder in terms of the (McNaghten) rules, because their terms are those of a conception of mental disorder which has long ago been abandoned, if, indeed, it was ever seriously held.' In actual practice the McNaghten rules work 'because they are an almost perfect example of that illogical, commonsense, tacit compromise to which the law constantly resorts when it is faced, as it is here, with an insoluble problem'. In the author's opinion, the question of fitness to plead should be given more attention at present than the McNaghten rules.

In a chapter on 'The bad old psychiatry' which Dr Yellowlees recalls with respect and affection, he includes some charming reminiscences of his earliest dealings with the insane in a mental hospital of which his father was medical superintendent.

He believes that mental hospital experience is necessary for the psychiatrist, and he thinks that the art of healing is as important as the technique, and that its essence lies in the personal relationship between doctor and patient. In his view, this art is at present in decline because psychiatry has come to depend too much on 'its amazing technique'. He vigorously criticizes the psychiatrists who exemplify this development, and he considers that the training of psychiatrists today is not all it should be.

These views will be read with great interest by workers in this field. But was the author wise to deal with these topics in a book meant for laymen? This Pelican will probably be read by thousands of people including sick men and women who are looking for help, and, although they will learn something about psychotherapy and other methods of treatment, these prospective patients may wonder to whom they should turn. For it would not be surprising if such readers formed the impression that the only psychiatrists to be trusted are those with the same kind of training and experience as Dr Yellowlees.

The author acknowledges psychiatry's debt to Freud, and his own description of psychopathology is largely Freudian. He scorns official psychoanalysis however, is sceptical about its therapeutic methods and reserves his heaviest irony for those who have had what he calls a 'complete Freudian psychoanalysis'.

The views of so able and experienced a psychiatrist are well worth hearing, and although Dr Yellowlees is not always objective (and presumably would not claim to be) he is invariably entertaining.

V. B. KANTER

On Human Thinking. By K. W. MONSARRAT. London: Methuen. 1955. Pp. 156. 15*s.*

Though slight in bulk this is an ambitious work which endeavours through a philosophical examination of human thinking to correct man's social errors and pave the way for social harmony and happiness. Dr Monsarrat, a medical man, whose earlier writings on human powers and human desires showed him to be thoughtful and original, was not badly qualified for his task and his views, though somewhat abstruse, are suggestive and interesting. Briefly he believes that impersonal, objective or scientific knowledge contains *a priori* features derived from our direct personal experience of power in action and temporal endurance, and that these direct personal intuitions contribute to a dynamic understanding of the world without, and enable man to comprehend social systems as organized systems in dynamic equilibrium. Unfortunately, man has misread the lessons of his own experience, and the source of his mistakes lies deeply in habits of thought. The author examines some of the erroneous modes of human thinking, philosophical, religious and political, and points the way to a mode of thought which integrates the evidence provided by experience and by history. It is in the role that he gives to inner personal experience in the interpretation of external reality that Monsarrat is most challenging, and most likely to interest psychologists.

L. S. HEARNSHAW

The Harvard List of Books in Psychology. Second edition. Harvard: University Press; London: Geoffrey Cumberlege. 1955. Pp. viii + 84. 10*s*.

This list contains six hundred and seven books selected to represent all areas of contemporary psychology. The books mentioned are those which have appeared up to March 1954, and each is briefly annotated. Although no bibliography of this sort can avoid arbitrariness, its quality is guaranteed by the fact that the compilers are members of the Department of Psychology, the Department of Social Relations, and the School of Education in Harvard University. It will interest those concerned with maintaining psychology libraries and with preparing reading lists to accompany courses in almost any of the numerous branches of psychology.

I. M. L. HUNTER

Situation. Utrecht: Spectrum. 1954. Pp. 260. No price given.

Situation is a collection of 'Contributions to Phenomenological Psychology and Psychopathology'; it is due to appear once every year, this being the first volume. The title is somewhat misleading. The book does not contain an account of phenomenological experiments in perception and other fields, but is full of essays of a belletristic kind on such subjects as 'Marriage and Love', 'L'Expérience Du Vide', 'On Hope' and 'The Physiognomy of Prognostic Uncertainty'. It is doubtful if psychologists (in the technical sense) would find anything of interest in this trilingual review, although it might help them to appreciate the enormous difference between Anglo-American and European concepts of psychology.

H. J. EYSENCK